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On the Functions of the Air-cells, and the Mechanism of Respiration in Birds. By W. H. Drosier, M.D. *Caius College.*

He remarked that, although, during the last two centuries and a half, a great many distinguished anatomists and physiologists (whose names he mentioned) had directed their special attention to the conformation of the respiratory organs of birds, and their functions, nevertheless our knowledge in regard to these matters was in a very unsatisfactory state. For instance, with regard to the number and relations of the large air-vessels in connexion with the lungs of birds, anatomists seem to be not yet agreed; and with respect to the uses of these, the views held by different physiologists were very contradictory, and at best only conjectural: all which shews that the subject in question was one of great difficulty.

It is not a little singular that the views, which have been chiefly relied on as explaining the principal uses of the air-cells and their continuations into the cavities of the hollow bones of birds, will not bear for a moment a close scrutiny. The first

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and principal of these was originated by Camper, who in 1771 discovered accidentally that the hollow bones of birds communicate with the great air-sacs; and conceived that the air contained in these, being warmed, lessened the specific gravity of the bird, and so rendered it fitter for flight. Since the time of Camper this view has been almost universally adopted. Thus Owen in a work just published adopts it[1]. In disproof of this view, the lecturer gave a calculation to shew that a pigeon weighing 10 ounces, or 4375 grains, would have its weight in air diminished by only a fraction of a grain in consequence of the rarefaction of the air in its air-sacs and hollow bones from the warmth of the body; so that the floating power resulting from such rarefaction would be almost inappreciable. We cannot then for a moment suppose that the extensive and complicated system of air-vessels has for a principal object a saving in weight so trifling : not to mention that this system is found in all birds, even such as do not fly.

Another theory that has been very generally adopted was propounded and developed by Cuvier[2], who assigned to the air-cells an office directly supplementary to that of the lungs; so that the air having passed through the lungs and renovated the blood in the pulmonary capillaries, is brought once again into contact with the blood in the systemic capillaries in every part of the body, and again exchanges oxygen for carbonic acid with the vital fluid. This theory was founded on a great anatomical mistake, into which, strange to say, many eminent anatomists have fallen, viz. that the air passes from the air-sacs into the cavities of the peritoneum, pericardium, and pleuræ, and even extends itself between the muscles, and beneath the skin: whereas it is in reality confined

[1] Owen, *On the Anatomy of Vertebrates*, Vol. II. 1866, p. 216 ; also Art. *Aves* in *Todd's Cyclop.*

[2] *Anatomie Comparée*, 1805, t. IV. p. 330, and Owen, l. c. cum multis aliis auctoribus.

within the membranous walls of the air-sacs, and the various definite prolongations of these. This latter was the description given two centuries ago by Harvey and Perrault, and more recently by Colas, Guillot and Sappey; and its correctness does not admit of a doubt. An interchange of gases may, indeed, take place between the air in the air-cells and the blood in the vessels of their walls: to a very trifling extent however; for the blood-vessels of these delicate membranes are very minute, and sparsely scattered. We cannot therefore but believe that the extensive, elaborate and (as regards the class of birds) ubiquitous system of air-cells must have some far more important office than that of conveying oxygen to these minute vessels. The question is, What is this office?

Bearing in mind that the air-sacs occur in all birds; that their number and position is very uniform; that they always have very free communications with the lungs; that the greater part of the air inspired passes into them, and is again expelled in expiration, we can scarcely doubt that they are a necessary part of the respiratory apparatus of birds.

The respiration of birds, even when in repose, has been shewn to be much more active than that of mammals. But in order that birds may be equal to the enormous exertion required of them for sustaining themselves in the air for considerable periods of time, very ample provision must be made for respiration. If therefore the lungs were constructed after the mammalian type, they would require to be very large, and powerful muscles must be provided for the respiratory movements. But this would add unduly to the weight of the body. The lungs therefore are small, very porous, and light; and yet nevertheless they are more efficient respiratory instruments than mammalian lungs of greater weight and volume. This we know from experimental comparisons of the quantity of oxygen absorbed, and of carbonic acid evolved by birds and mammals, for each pound weight of their bodies, in equal times. The increased efficiency is due to

three causes; first, to the greater rapidity of the circulation, on account of which more blood passes through the lungs in a given time; secondly, to the more abundant supply of air drawn in at each act of inspiration; thirdly, to the more complete exposure of the blood in the pulmonary capillaries of a bird to the action of the respired air. It must be observed, that it is necessary that these three conditions should all be satisfied at the same time, to make the lungs more perfect as respiratory organs. For if more blood pass through the lungs in a given time, it would be of no avail unless more fresh air were at the same time supplied for its aeration. And again, if these two conditions were satisfied, the quicker transit of the blood would necessitate the provision of increased facility for interchange of the gases by osmose through the walls of the pulmonary capillaries; otherwise the blood would pass on before hæmatosis was sufficiently effected. Now in the bird the first of these conditions is satisfied by the more rapid action of the heart; the second by the greater quantity of air inspired; the third by the peculiar structure of the lungs, in which, as was first pointed out by Mr Rainey, in 1848, the whole circumference of the almost naked pulmonary capillaries is exposed to the action of the air in the lungs, which passes between and around them, in the areolæ that answer to the cells of the mammalian lung.

The lungs of birds being very small cannot contain much air. When however the ample chest expands, a large quantity of air rushes in by the trachea, and passing through the larger bronchi, that open into the great air-sacs, fills them. It has long been known that the air-sacs do not all expand at the time of inspiration, but only the two pairs that lie at the sides of the thorax, and are in contact with the ribs and the intercostal muscles: these may be called the middle sacs. The others, namely, the abdominal, the cervical, and the anterior thoracic (which may be called the extreme sacs) contract at this time. In expiration the action of both sets of cells is reversed. During the time of inspiration,

then, the middle sacs become filled with air. The pulmonary diaphragm is at this time in a state of contraction, and its tense aponeurosis, which is attached to the inferior surface of the lungs by a fine cellular tissue, causes the lungs to expand to such an extent as they are able, for they are not very elastic. In so expanding they receive a part of the inspired air, which however does not remain stationary in them, but passes on with a gentle motion towards the middle air-cells, in which the pressure has been reduced by the expansion of the walls of the thorax. A much greater amount of air however reaches the middle air-cells by the more direct, and more open course of the larger bronchi, that open into them; and a third portion passes into them, at the same time, from those extreme cells that are nearest to them, and which are contracting, as we know, at this time. Inspiration being completed, expiration commences, and endures for a longer time than inspiration. The pulmonary diaphragm relaxes, and in consequence of the partial closure thereby of certain openings, the middle air-sacs slowly empty themselves by openings in part differing from those by which they were filled; so that the air passing through the smaller bronchi and the substance of the lungs, goes principally out by the trachea, but partly into the extreme air-sacs, which at this time are dilating. Hæmatosis is going on therefore during expiration as well as during inspiration; so that birds are truly animals having a *double respiration*, though not in the sense in which Cuvier so defined them[1].

It is exceedingly instructive to observe the way in which the organization of birds has been modified to meet the requirements of their marvellous powers of rapid and long-sustained flight. Muscles capable of very powerful and rapid action are necessary to support the bird in the light and yielding air. This again requires very ample provision for respiration. The amount of muscular force must also be proportioned to the weight of the

[1] Cuvier, *Règne Animal*, 1817, t. I. p. 290.

animal to be sustained in the air. If we can diminish the weight of the osseous system, we may lessen, in a *greater* proportion, the weight of the muscular; for there will be less muscle, as well as less bone to carry. In birds the osseous framework is made as light as is consistent with the strength necessary, by reducing as far as possible the quantity of osseous matter, and so disposing this on mechanical principles as to offer the greatest possible resistance to the action of the muscles. To this end the bones are hollow, and the bony matter deposited in superficial lamellæ, dense, but of no great thickness. Air is admitted into their cavities, to absorb the aqueous matter secreted by the endosteum, and being renewed gradually by the respiratory movements, carries off the moisture that would otherwise collect, fill the cavities of the bones, and add greatly to their weight. The cellular tissue of the lungs is reduced to a minimum, with the double effect of rendering them lighter, and facilitating hæmatosis; and, finally, voluminous air-vessels, of great tenuity, and containing large supplies of air for respiration, without materially increasing the weight of the body, conduce to make the respiratory apparatus of birds one of the most striking examples of the perfect adaptation of means to ends to be met with in the animal kingdom.

Our great physiologist, John Hunter, believed it impossible that the ribs and sternum of a bird could move while the powerful pectoral muscles are engaged in flight. He therefore thought that the air-sacs of birds might be intended to act as reservoirs of air to be used in respiration during flight. These sacs, however, do not hold enough air to support the respiration of a bird for two minutes; for in that time, if the trachea of a bird be tied, it dies; yet many birds continue on the wing for hours together. Sappey has endeavoured to explain the difficulty of Hunter by pointing out that the great pectoral muscles of birds arise exclusively from the sternum, and not at all from the ribs, as they do in mammals. But this explanation only removes

a part of the difficulty; for the ribs are so articulated with the sternum, that they cannot move unless the sternum moves also. Now the sternum in respiration moves at its articulations with the two coracoid bones, these bones being fixed in regard to the sternum and humerus in the movements of flight. It might seem, therefore, that when the pectoral muscles contract, the sternum would be drawn powerfully upwards as the wings are drawn downwards, and so the sternum and ribs fixed. But this is not so; for the fibres of these muscles converge towards and pass over the coracoid bones on their way to be inserted into the ridge of the humerus, and they act, when the wing is extended in flight, in a line very nearly parallel to the axis of the coracoid, but a little below it. Their principal effect on the sternum, therefore, is to draw it more closely against the coracoid; so that they do not much interfere with the action of the respiratory muscles, rather assisting however the inspiratory muscles in depressing the sternum; a circumstance favourable to deep inspiration during flight.

The author gave a mathematical as well as an experimental proof that the external intercostal muscles raise both the ribs to which they are attached, and that the internal intercostals depress both ribs. A frame of wood, in the form of a parallelogram with hinges at the angles, represented two ribs, the spine, and the sternum. An india-rubber ring was passed over a peg in the upper rib and another in the lower rib, at different distances from the spine, to represent the intercostal muscle. Both ribs were elevated or depressed according as the upper peg was nearer to or further from the spine than the peg in the lower rib.

Dr HUMPHRY made a few remarks upon some points in the paper, expressing himself not quite satisfied with the proof of the aeration of the blood by means of the double current of air from the cells.

I. Todhunter, M.A., F.R.S., Vice-President, in
the Chair.

The following new Fellows were elected :

B. W. Beatson, M.A., *Pembroke.*

J. R. Lumby, M.A., *Magdalene.*

F. J. Braithwaite, M.A., *Clare.*

On the Papyrus of the Lake of Gennesaret. By
Charles C. Babington, M.A., F.R.S., *Professor
of Botany.*

The object of this communication was to point out the ex-
istence of two species of *Papyrus*, and to explain that the
plant in cultivation as the " Egyptian Papyrus " is not the true
Cyperus Papyrus (Linn.), but was probably introduced into our
gardens from Sicily, where it had been planted (in the opinion
of Prof. Parlatori of Florence) shortly before the 10th century
of our era.

It was also shewn that the Papyrus of the plain of Genne-
saret (from whence a specimen gathered by Mr Tristram was
exhibited), and of the vast and deep marshes of Hûleh, by the
Lake Merom, in the north of Palestine, is not the plant found
at Syracuse and on the coast of Palestine, but is identical
with that which grows in an extensive swamp or shallow lake
connected with the White Nile at about the seventh degree of

north latitude. Also that Bruce found it at the same place in Palestine, and in two lakes of Abyssinia.

As the plant bears a heavy head upon a lofty stem, and does not root strongly in the ground, it can never have been common in Egypt, for the high winds and strong current of the river would be too powerful for it. It was probably brought to Egypt chiefly from Nubia, and only grew in the marshes and back-waters of the river when within the true limits of Egypt.

The communication concluded with a few remarks upon the technical characters of the plants derived chiefly from Parlatori's paper in the *Memoires of the Institute of France.*

PROFESSOR LIVEING exhibited an echinoderm from the coralline crag of Aldborough, which he referred to the genus Rhyncopygus (D'Orbigny). Two imperfect specimens of the same species from the red crag have been figured and described by Forbes, who referred them doubtfully to the genus Echinarachnius, but these specimens evidently did not shew the peristome.

Professor Liveing's specimen is a depressed urchin, convex above, concave below, the concavity shovel-shaped, the posterior lobes being more developed and descending to a greater distance below the mouth than the anterior lobes. The apex sub-central, somewhat anterior, dorsal ambulacra sub-petaloid, the poriferous zones nearly parallel, extending nearly to the margin and open. The mouth (which in the specimen is partly crushed and one side gone) sub-central but somewhat anterior, the ambulacra about the mouth sharply defined, leaf-like, shallow depressions, with crenate margins, the interambulacral spaces terminating in small tubercles. The anal opening marginal, transverse and overhung by a projection of the back. Genital pores somewhat large and three in number, the left anterior pore wanting. The whole test covered with thick-set tubercles,

each surrounded by a depressed areola and a circle of granules. Some of the tubercles shew a ligamental cavity in the centre.

The want of the 4th oviduct may be only an individual peculiarity, as some recent species are known of which individuals have 2, 3, and 4 genital pores respectively; and in other respects the characters of the specimen agree with those of the genus Rhyncopygus, except as to the position of the anal opening, which in the other species of that genus is supra-marginal. With respect to this character, as soon as the perfectly radiate type in which the opening is in the apex is departed from, it is merely a question of degree, whether the opening be higher or lower in the posterior interambulacral space, and no connexion is known to exist between the position of the opening, when excentric, whether marginal or extra-marginal, with any important modification of other parts. The margin is not defined by any anatomical characters, and moreover in some tolerably well defined genera the anal opening is found to vary in position in different species. Now we ought not to take as a generic character any external peculiarity which is not either known to be connected with some important modification of the anatomy of the creature, or which does not belong to some feature which in some well-defined genera is so constant as to lead us to suppose that the law of the creature's development will not permit that feature to vary without an otherwise important change in the anatomy of the animal; the position of the anal opening with reference to the margin satisfies neither of these conditions, and for these reasons Prof. Liveing considers the marginal position of the anal opening an insufficient reason for separating the specimen from the genus Rhyncopygus. The other species of this genus named in Desor's Synopsis are cretaceous, except one from the quaternary deposits of Guadaloupe, so that the occurrence of this genus in the crag is a link rather with the older than the more recent formations.

*On a new Theory of the Skull and of the Skeleton; with
a Catalogue of the Fossil Remains of Vertebrate
Animals contained in the Woodwardian Museum.
By* H. SEELEY, F.G.S., *Sidney Sussex College.*

THE author thought that it was not possible to discuss the
theory of the Skull till the theory of the Vertebræ had been
determined; and that it was impossible to arrive at a theory
of a vertebra without considering the theory of the growth
of a simple ossification; and believed that when the theory
of ossification was once arrived at, then the theory of the verte-
bra, the theory of the skull, and the theory of limbs would
follow from the conditions which determined the multiplication
and co-ordination of simple bones.

First, then, he endeavoured to show what the possible me-
chanical forces acting upon bones were; and having discussed
the source of these forces, concluded that all growth must be
due to vibrations of pressure and tension; and that the intensity
of growth depends on the amount of pressure and tension in the
direction of the increase; and that the bones owe the origin of
their first particle and form to the same causes which add to
their bulk: illustrations of these views being drawn from Me-
chanics, Pathology, and Comparative Anatomy.

Secondly. The nature of compound bones was considered,
as illustrated by the comparative osteology of the humerus, and
by the carapace of the turtle, and the so-called tarso-metatarsal
bone of birds. And the author arrived at the conclusion that a
primary ossification might, if the pressure and tension were
sufficient, develope upon itself other ossifications or epiphyses
in any direction; and that these epiphyses might themselves
assume the nature of separate bones and also develope epiphyses:

or, in other words, one ossification may develope another if suffi-
cient pressure and tension can be applied to its surface : a law.
which appeared to be as true for the entire animal, as for a
single bone.

And hence the vertebra was supposed to consist of a centrum
or centre of ossification which typically developes three pairs
of epiphyses : one pair, in front and behind, being the epiphyses
commonly so called; another pair above to enclose the neural
cord, called neural-epiphyses; and a third pair beneath to en-
close the viscera, called hæmal epiphyses. The author then
discussed the extent to which these epiphyses actually do pro-
duce other epiphyses.

And *thirdly*, the comparative osteology of the skull, and
the morphological and embryonic development of the brain were
considered ; and the conclusion drawn, that since the skull
was the entrance to three distinct regions of the body, the ner-
vous system, the respiratory system, and the digestive system, it
must be considered in the relation of its three several regions to
the corresponding parts of the organism before it can be com-
pared with the body as a whole.

The author then considered the ossification of the trachea,
and showed how by the laws which had been arrived at, ossifica-
tion would be greater at its termination where it is in contact
with other bones; and so the circles of bones which surround the
anterior termination of the respiratory region in the skull were
regarded as only the modified end of the trachea, and therefore
could not correspond with any part of an ordinary vertebra.

The brain-case was recognized as consisting of three seg-
ments; and such that the basisphenoid corresponded to an
ordinary vertebral centrum, and developed the presphenoid and
basi-occipital for its epiphyses in one direction, and the alisphen-
oids in another; and by a law previously determined it was
considered that the whole occipital segment was the posterior
epiphysis of the whole parietal segment, while the whole frontal

segment was its anterior epiphysis. The brain-case therefore being modified from the plan of a single vertebra.

The lower jaw, both by homology in embracing the respiratory and digestive systems and by development, would be the hæmal epiphysis or representative of a rib to the parietal segment of the skull.

In conclusion, the author regarded the skull as the terminal segment of the body, each of its three regions being modified on the plan of the corresponding structures of the adjacent and continuous regions of the body.

March 12, 1866.

The PRESIDENT (W. H. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The following new Fellows were elected:

W. J. BEAMONT, M.A., *Trinity.*

N. GOODMAN, B.A., *St Peter's.*

On the Homeric Tumuli. *By* F. A. PALEY, M.A.

THE object of this paper was to shew that most of the facts which have been ascertained from the exploration of sepulchral barrows in this and other countries may also be verified historically, and with considerable minuteness, from the Homeric poems, in which much that appears authentic is said about the tumuli of the Grecian and Trojan heroes, and also about the ceremonies of interment. These facts were collected and compared with a view of shewing the very great antiquity

of this method of sepulture, which prevailed for ages, and apparently when no other method was in vogue, all over the north of Europe, and especially in the neighbourhood of the Euxine and about the Crimea; and it was also shewn that barrows existed which were regarded as of unknown antiquity even in the Homeric ages. The burning of the body of the deceased, the collecting of the bones in sepulchral urns, the burial of the arms or other ornaments in the same mound, the method of making the mound, with or without a circle of foundation-stones, and the surmounting of the tumulus itself by a *stelè*, or stone pillar, were all described from Homer; and it was shewn that in all these respects the written accounts generally agreed in a remarkable manner with observed facts. Sir W. Gell's identification of the Homeric tumuli with existing barrows was discussed; the etymology and significance of several of the Greek words relating to this form of burial were explained, and the institution of games, with the sacrifice of living victims to the spirit of the deceased, illustrated by examples. And a comparison was made of the two detailed accounts in *Iliad* XXIII. and XXIV. of the funeral rites performed at the burning and burial of the bodies of Patroclus and Hector respectively. Lastly, the shape of the ancient barrows, whether oval or circular, was considered, in reference both to the expressions in Homer, and to modern examples in the Troad, in Sweden, and elsewhere. The occasional use of ancient tumuli as land-marks, as posts of observation, or as places for holding councils, was proved from passages in Homer; and some points were suggested as worthy of particular notice in the event of ancient barrows being explored.

PROFESSOR SELWYN inquired whether any differences had been observed in the forms of existing tumuli, according to their positions, which would strengthen the arguments for their having been used as land-marks; he also made some remarks upon the connexion between trees and tumuli.

Mr BONNEY (St John's) mentioned some tumuli in Brittany, Scandinavia, and Constantine (Algeria) which illustrated the use of stelæ, and the method of building the mound. He discussed the probable antiquity of some in the first country, and shewed that they belonged in all probability either to the Neolithic or to the Bronze age, exhibited a progress in Art, and were certainly pre-Roman. He also described the various forms of their tomb chambers, and made some remarks upon the geographical distribution of these remains.

The PUBLIC ORATOR (Mr W. G. CLARK, Trinity) gave some interesting particulars from his personal recollections concerning the barrows in the Troad, and mentioned that all which had been explored lately had been found to have been previously opened. He also commented upon the difficulty of deciding upon the age of tumuli from the remains contained, owing to the fact that one material did not necessarily wholly replace another in the manufacture of weapons, &c.; instancing the heaps of flint chips at Marathon, which are comparatively modern, being the remains of threshing instruments still used in agriculture in that part of the country. He also doubted whether the various ' ages' could always be maintained.

In reply to a question by Mr Candy (Sidney) as to the relation of Mr Paley's paper to Stonehenge, Professor CARDALE BABINGTON briefly indicated the commonly received opinions concerning the date and purpose of Stonehenge; and brought forward instances from Denmark and the north of Europe to shew that the distinction of the ' ages' could not be entirely set aside.

On the Method of demonstrating some Propositions in
Dynamics. By I. TODHUNTER, M.A., F.R.S., *St John's*
College.

SUPPOSE a particle moving in a straight line; let *s* be the
space described at the end of the time *t*, *v* the velocity, *f* the
acceleration; then we have the equations

$$f = \frac{dv}{dt}, \quad f = \frac{d^2s}{dt^2}.$$

And similar equations hold in more complex cases of motion.

Thus we have theoretically the choice of two methods when
we wish to determine *f*; namely we may first find the velocity
and then *f* from the relation $f = \frac{dv}{dt}$; or we may find *f* without
attending to the velocity from the equation $f = \frac{d^2s}{dt^2}$.

The propositions given in Newton's second and third sec-
tions are in effect treated in the latter of the two methods. It
is however quite possible to treat them by the former method;
and the following advantages seem to follow by adopting the
first method.

The results can be obtained without requiring so much
knowledge of the properties of Conic Sections.

The theory of limits is used in a more simple and convincing
manner.

The illustration of mechanical principles is more varied.

As an example take the most important proposition, namely
that of motion in an ellipse round the focus.

The figure may be easily constructed. Let *S* be the focus
of force, *H* the other focus, *P* and *Q* two points on the ellipse.

It may be shewn in the most elementary manner that the

velocity at any point can be resolved into two, perpendicular to the radius vector and axis major respectively; and both constant. Call these v_1 and v_2 respectively.

Now suppose the body to move from P to Q; when it arrives at Q it has velocities equal and parallel to those which it had at P, together with the velocity generated by the central force during the motion from P to Q. Call this last velocity u, and suppose its direction to make an angle ψ with SQ. Let $PSQ = \phi$.

Thus at Q we have three component velocities in assigned directions; and these must be equivalent to v_1 and v_2 perpendicular to SQ and the major axis respectively. Hence

$$v_1 \sin \phi - u \cos \psi = 0,$$
$$v_1 \cos \phi + u \sin \psi = v_1;$$

therefore $$v_1 \left(\cos \phi + \frac{\sin \phi \sin \psi}{\cos \psi} \right) = v_1;$$

therefore $$\cos \psi = \cos (\phi - \psi);$$

therefore $$\psi = \tfrac{1}{2}\phi.$$

This result is *exact*; it shews that as the body moves from P to Q the effect of the central force is to generate a resultant velocity the direction of which bisects the angle PSQ.

Let f denote the acceleration at P, then when the time, t, of motion is made small enough we have

$$ft = u,$$
$$\tfrac{1}{2}ht = \frac{r^2 \sin \phi}{2}, \text{ where } r \text{ is put for } SP;$$

therefore $$fr^2 \sin \phi = hu$$
$$= \frac{v_1 \sin \phi}{\cos \psi},$$

or ultimately $$f = \frac{v_1}{r^2}.$$

This shews that the force varies inversely as the square of the distance.

It will be seen on examining this demonstration that it involves very few properties of the ellipse, and those only of the most elementary kind. It introduces an important result, namely that involved in the relation $\psi = \frac{1}{2}\phi$. And the theory of limits is only required in a form which may be easily understood and admitted.

After arriving at the result $\psi = \frac{1}{2}\phi$ we might complete the demonstration thus: Let PSQ be any finite angle as before; let p be adjacent to P and q to Q, such that the angles PSp and QSq are equal, and p and q fall between P and Q.

Then since the angles pSq and PSQ are bisected by the same straight line, the central force produces as much effect while the body moves from P to p as it does while the body moves from q to Q. But the times of describing these portions are ultimately as SP^2 to SQ^2; and therefore the forces at P and Q are ultimately as $\dfrac{1}{SP^2}$ to $\dfrac{1}{SQ^2}$.

PROFESSOR ADAMS made a few remarks on this communication, describing a somewhat similar investigation which he had used in his own Lectures.

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On Capillary Attraction. *By* RICHARD POTTER, M.A.

THIS being a mathematical paper, it admits of only a very imperfect abstract. For the history of the subject up to 1834, reference was made to the very complete report drawn up by Professor Challis at the request of the British Association, and published amongst their reports.

After noticing some points in the history of the subject, such as M. Clairaut's proposition of the relation of the attraction of the solid for the particles of the liquid in contact with it, compared with the attraction of the liquid for the same particles, in order that there may be capillary elevation or depression of the liquid; the views of Segner, Monge, and Dr Young, that the phenomena were due to the tension in a flexible sheet forming the capillary surface; and the mathematical discussions of Laplace and Poisson; the author stated his agreement with those who considered these latter as only having obtained their results in accordance with Dr Young's hypothesis of a constant tension in the sheet of inappreciable thickness forming the capillary surface.

MM. Frankenheim, Sondhaus and Brunner have shewn the phenomena of capillary attraction to be functions of the temperature and not of the density of liquids.

The author stated that he had employed no new principle in his method of solution of the problem of the forms of the capillary surfaces, but had employed in a different manner the recognized properties of fluids, and treating each case as a distinct hydrostatical problem, solutions were obtained in the first instance for the cases of vertical parallel plates *near together*, and tubes of *small diameter*, with their lower ends immersed in liquid, by the statical property of forces acting on bodies, that the vertical and horizontal forces must balance each separately amongst themselves; and that a fluid by Pascal's principle may be considered as separated into distinct portions by imaginary rigid films without the state of the fluid being altered. The vertical force exerted along the line of contact of the solid and liquid is shewn by Laplace to be the force which supports the weight of the liquid in capillary elevations above the level of the outside fluid, and the same must be the case when the liquid is separated by imaginary rigid films into portions likewise. This vertical force is balanced by the vertical component of the tension in the capillary surface of the liquid at the line of contact. The tension which may exist in the surface is limited by the attraction of aggregation from which it must be distinguished, as it is a force transmitted from the impressed force at the line of contact, and as in flexible solids, such as cords and sheets, it may vary from nothing to the utmost the attraction of aggregation permits. The horizontal forces are only the horizontal components of the tension, which must therefore balance amongst themselves as in the catenary curve. For the capillary depression the procedure is the converse of that for capillary elevations.

With these considerations it was found that between vertical parallel plates for a first approximation the form of the perpen-

dicular section of the capillary surface was a parabola, from which a second approximation furnishing a small correction of the first was easily obtained. In tubes of small caliber a like method shewed the section of the capillary surface by a vertical plane through the axis to be a parabola, as a first approximation, with a small correction of the result as a second approximation.

Success with these cases led the author of the paper to investigate the form of the capillary surface for a liquid of indefinite extent in contact with a single plane vertical surface of a solid of which the lower edge was immersed in the liquid. This is the first case treated in the paper as the fundamental one of capillary attraction. By taking an elementary vertical prism in the liquid held above the original level, and finding the conditions for equilibrium amongst the forces acting at its upper surface, a differential equation for the tension was obtained, and from the consideration that the tension must be the minimum possible at each point when the liquid is at rest, the calculus of variations gives the form of the section of the surface by a vertical plane perpendicular to the plane of the body as an exponential curve with the equation $z = h \cdot \epsilon^{-\frac{y}{k}}$, where h and k are constants, z the vertical and y the horizontal ordinate of any point in the surface, with the origin the point where the vertical solid meets the level of the liquid at a great distance.

An addendum to the paper contained investigations for the forms of the capillary surfaces between vertical parallel plates at *any distances* and for tubes of *any diameter* without approximation. For parallel plates at *any distances* the equation of the section of the surface by a plane perpendicular to them was found to be expressed in finite terms as follows:

$$z = \frac{h'}{2} \{\epsilon^{\frac{y}{m}} + \epsilon^{-\frac{y}{m}}\},$$

where h' and m are constants, z and y the vertical and hori-

zontal ordinates of any point in the surface, with origin a point equally distant from the plates, and the axis of y perpendicular to them in the level of the outside liquid. For tubes of *any diameter* held vertically in a liquid a differential equation was found and a relation between the ordinates expressed in an infinite series, but an equation in finite terms was not found. The equation for parallel plates is similar to that for the catenary curve for a uniform chain, but differing from it in having the constants different from each other.

The first terms of the expansions in series of these two cases give the results which were before found by approximate methods.

In the discussion which followed, PROFESSOR CHALLIS made some remarks upon the difficulty of the subject which Mr Potter had been investigating, but reserved his approval of the method followed until he could give the argument a fuller consideration. PROFESSOR STOKES objected to the method pursued, and was unable to agree with the results obtained.

May 7, 1866.

PROFESSOR STOKES (Senior Member of the Council present) in the Chair.

On the Root of any Function; and on Neutral Series, No. II. By Prof. DE MORGAN.

THE author divides algebraical thought into *quantitive* and *structural*. A quantitive proposition is seen in 'A value of x can be found, so that $\phi x = a$': a structural proposition in 'If $y = \phi x$, there exists a form, ϕ^{-1}, such that $x = \phi^{-1} y$.' These

propositions are co-extensive : but the first is never either affirmed or denied; the second is always tacitly assumed. Mr de Morgan sets out as much of the differential calculus as he wants, from quantitive considerations alone : he demands nothing but values of his functions, and does not need to know even the form of the differential coefficient of a product.

Argument on difficulties apart, such difficulties as are always watched with keen eyes by those who examine a new theorem as it ought to be examined, the proof that every function has a root is so simple that it is its own abstract.

If $\phi(x + y\sqrt{-1}) = P + Q\sqrt{-1}$, and if $dP : dx$ be represented by P_x, &c., we have $P_x = Q_y$, $P_y = -Q_x$. It is easily shewn that the families of curves $P_x = h$, $Q_x = k$, are *orthogonal trajectories;* that is, individuals, one of each family, always meet at right angles. Here h or k may be nothing or infinite. Consequently, P_x and P_y cannot both vanish, or both become infinite, except at isolated points. And it is easily shewn that one of the two, without the other, can only become infinite at isolated points. Hence a curve may be drawn from any one point to any other, so as not to pass intermediately through any point at which there is impediment to simple quantitive reasoning upon the equations

$$dP = P_x dx + P_y dy, \quad dQ = -P_y dx + P_x dy,$$

or their consequences $(R = P_x^2 + P_y^2)$

$$R dx = P_x dP - P_y dQ, \quad R dy = P_y dP + P_x dQ.$$

Let it be required to solve $\phi(x + y\sqrt{-1}) = a + b\sqrt{-1}$; or to find x and y so that $P = a$, $Q = b$. Let $x = \lambda$, $y = \mu$, give $P = l$, $Q = m$; and, P and Q being co-ordinates, draw a curve which shall avoid all impediments from the point (l, m) to the point (a, b). Divide $a - l$, $b - m$, into

$$d_1 P + \ldots + d_n P, \text{ and } d_1 Q + \ldots + d_n Q,$$

as conceived in a common integration, each value of dQ being obtained from the (P, Q) curve by means of a value of dP.

From λ, μ, d_1P, d_1Q calculate d_1x, d_1y: from $\lambda + d_1x$, $\mu + d_1y$, d_2P, d_2Q, calculate d_2x, d_2y; and so on. We thus arrive at

$$\lambda + d_1x + \ldots + d_nx = \alpha, \quad \mu + d_1y + \ldots + d_ny = \beta,$$

the values of x and y which solve the required equation. This proof involves, though in a laborious way, the approximate determination of the value of $x + y\sqrt{-1}$ required, by use of small instead of infinitely small subdivisions.

The part of the paper which relates to neutral series is reconsideration of some difficulties connected with the method of the former paper on the same subject; it admits of no separate abstract. The chief point is the double approach of

$$a_0 - a_1 + a_2 - a_3 + \ldots + a_{2n} - a_{2n+1}$$

to the limiting form

$$1 - 1 + 1 - 1 + \ldots \textit{ad inf.}$$

May 21, 1866.

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

Notes on the Cetacea which have lately been taken on the Welsh coast, and on some other additions to the Museum of Comparative Anatomy. By J. W. CLARK, M.A.

AFTER a short sketch of the present state of knowledge of the Cetacea, and a few words on the difficulty of obtaining specimens in a perfect condition, Mr Clark described some examples of Delphinus Tursio which had recently been obtained for the Anatomical Museum at Cambridge. These were from a shoal of sixteen which came ashore near Holyhead, of which two

tolerably perfect examples had been purchased, parts of which were exhibited. In their stomachs were found whelks, crabs, and a considerable number of pebbles; and in one a conger eel. He drew attention to the fact that the teeth were not universally blunted as it had been stated.

On certain alleged misrepresentations and discrepancies in Plato's Theætetus. (GROTE'S *Plato*, Vol. II. c. xxvi.) *By* E. M. COPE, M.A.

THE Author commences by expressing the obligation which he, in common with all scholars, feels toward Mr Grote for the light which, by his extensive learning and independent criticism, he has thrown, in general, upon the Platonic doctrines and writings.

Plato stood in especial need of an impartial statement and thorough sifting of the doctrines attributed to him; owing to the preconceived notions which had so influenced those who had undertaken the task of interpreting him, that students were too often led to look for what Plato might have said or ought to have said rather than what he did say.

Mr Cope gives two instances of these bars to the right understanding of Plato.

(1) The theory held by Schleiermacher, that Plato's dialogues are not only all pervaded by one spirit and tone of feeling, but also can be fitted together so as to form essential parts of one complete system.

(2) A tendency to unduly harmonize and interpret by inference rather than by the direct statements of the author, and so to represent the miscellaneous collection of the Platonic

dialogues as one body animated by one soul, as a harmonious whole, pervaded in all its parts by a central unity of purpose governing the whole.

Mr Cope then proceeds to state the professed object of the Theætetus : viz. the determination of the nature and characteristic distinctions of knowledge as compared with other modes of apprehension belonging to the human intellect, namely, sensation or sensible perception; opinion or belief, δόξα or ἀληθὴς δόξα; or this accompanied by λόγος. He considers it to be the first serious attempt at a psychological analysis of the faculties or modes of apprehension of the mind, of which it suggests that there are three, sensation, belief, knowledge.

Mr Cope maintains that the question involved in this discussion of the Protagorean dictum 'πάντων μέτρον ἄνθρωπος' is no mere dialectical encounter of wits, but one of the highest interest, namely, "Is there any such thing as truth? if so what is it? is there any standard of truth and knowledge independent of ourselves our own feelings and momentary consciousness?"

Mr Cope then proceeds to consider the following questions :

(1) Has Plato misrepresented Protagoras' theory?

(2) Has he refuted it, or is it really true as Mr Grote holds it to be when properly interpreted?

(1) Mr Cope argues against the probability of Plato having misrepresented Protagoras' theory either wilfully or through ignorance, by a close examination of the discussion in the Theætetus; and further brings forward evidence from Aristotle, Diogenes Laertius, Sextus Empiricus, and Simplicius, to prove that he has not done so. Mr Cope then shews, from an examination of the sense in which words expressive of mental faculties or processes are used in the Theætetus, that there is an *a priori* probability that Protagoras was ignorant of any distinction between sensation and thought or knowledge; and therefore confined his theory of the subjective standard of truth to the apprehension of objects by sensation.

(2)   Mr Cope examines Plato's arguments against Protagoras which are mainly these :

(*a*) Protagoras asserts that the views, opinions, beliefs of everyone are true to himself as he conceives them ; but the majority of mankind think Protagoras' theory wrong ; therefore this opinion is true to them and yet is contradictory to the theory ; therefore the theory is in conflict with itself and so is false.

(β) Even if there were no other than the subjective standard of *present* truth and *present* right, there must be of *future ;* for in that all men's opinions and thoughts are not equally true and valid.   The man of science can, in certain cases, predict what will happen in future time—the ἰδιώτης cannot; his fancies and impressions are here of no value ; and are liable to be false even to himself.

Mr Cope developes these arguments and shews why he considers them to be satisfactory.

The paper concludes with a defence of Plato against a charge of self-contradiction in the statements and views of the Theætetus and Sophist on the subject of ' false opinion.'

The MASTER OF TRINITY made a few remarks; in the course of which he stated that, in his opinion, the ideas of Protagoras took a somewhat wider range than the senses alone; and that Plato was not primarily attacking Protagoras in the *Theætetus*, but some contemporary Sophist.

NOTICE.

AT a Meeting of the Council of the CAMBRIDGE PHILOSOPHICAL SOCIETY, *Nov.* 12, 1866, the following Resolution was carried :

"That every Fellow of the Society, who has paid his subscription for the year, or the composition in lieu of annual payments, be entitled to receive a copy of every Part of the *Transactions* published during the year to which his subscription refers."

Any Member of the Society can have his Part on personal application, or on sending an order (with stamps for postage, if necessary) to one of the Secretaries.

*** *A Part will be ready at the commencement of the Lent Term*, 1867.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

October 29, 1866. (ANNUAL GENERAL MEETING.)

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The following Officers were elected for the ensuing year :

President.

Rev. H. W. COOKSON, D.D., *Master of St Peter's College.*

Vice-Presidents.

G. E. PAGET, M.D., *Caius College.*
Rev. JAS. CHALLIS, M.A., *Plumian Professor.*
G. G. STOKES, M.A., F.R.S., *Lucasian Professor.*

Treasurer.

Rev. W. M. CAMPION, B.D., *Queens' College.*

Secretaries.

C. C. BABINGTON, M.A., F.R.S., F.L.S., *Professor of Botany.*
G. D. LIVEING, M.A., *Professor of Chemistry.*
Rev. T. G. BONNEY, B.D., F.G.S., *St John's College.*

The following were elected new members of the Council :

F. A. PALEY, M.A., *St John's College.*
I. TODHUNTER, M.A., F.R.S., *St John's College.*
J. W. CLARK, M.A., *Trinity College.*

The following continue Members of the Council :

A. NEWTON, M.A., F.L.S., *Professor of Zoology.*
G. M. HUMPHRY, M.D., F.R.S., *Professor of Anatomy.*
A. CAYLEY, M.A., F.R.S., *Sadlerian Professor.*
Rev. H. A. J. MUNRO, M.A., *Trinity College.*
Rev. G. F. BROWNE, M.A., *St Catharine's College.*
Rev. W. SELWYN, D.D., *Margaret Professor of Divinity.*
Rev. W. G. CLARK, M.A., *Public Orator.*
R. POTTER, M.A., *Queens' College.*
Rev. N. M. FERRERS, M.A., *Caius College.*

4—2

Note on the Halo of 22⁰. *By* W. H. Miller, M.A., *For. Sec. R. S., Professor of Mineralogy in the University of Cambridge.*

It had long been conjectured that the halos of 22° and 46° were produced by the refraction of the light of the sun through prisms of ice descending slowly through the atmosphere. The physicists who undertook to compare this hypothesis with observation assumed the value of the refractive power of ice, and in some cases they also assumed the existence of forms in ice which had never been observed. M. Bravais in an exhaustive Memoir on Halos, inserted in the 31 *Cahier du Journal de l'École Polytechnique*, and in *l'Annuaire Météorologique de la France*, Année 1851, gives an account of a very careful determination of the refractive power of ice made by himself, and shows that the radius of the halo of 22° agrees with sufficient accuracy with the minimum deviation of light through the alternate faces of the regular six-sided prism 0 1 1, and that of the halo of 46° with the minimum deviation through a face of the form 1 1 1, and a face of the form 0 1, $\bar{1}$, faces making a right angle with one another. By measuring the minimum deviations through a prism of ice, Bravais obtained for the indices of refraction the following values :—

Middle of red .... 1·307,
Middle of orange .... 1·3085,
Middle of yellow .... 1·3095,
Middle of green .... 1·3115,
Middle of blue and indigo 1·315,
Middle of violet .... 1·317.

As the observation presents many difficulties, and the value of the research depends entirely on the accuracy of these

numbers, it may be worth while to compare one of them, the yellow or the brightest part of the spectrum, with the value obtained by myself in a different manner.

A prism cut from a thick plate of ice, with its refracting edge normal to the surfaces of the plate, was placed in, a vessel having parallel ends of plate-glass filled with water obtained by melting a portion of the same plate of ice, to a depth of twice the thickness of the prism. The vessel containing the prism floating in water was placed so that a distant slit having a salted spirit flame behind it could be viewed with the telescope of a theodolite through the compound prism of ice, water and the plates of glass, and the image of the slit bisected, the prism was then depressed to the bottom of the vessel, and the slit as seen through the water and glass plates alone bisected. In this way the refraction out of ice into water was obtained unaffected by any small error in the determination of the angle of the ice prism, the impurity of the ice, and the want of perfect parallelism of the plates of glass. The resulting index for the line D out of ice into water at 0° was $1{\cdot}01952$. According to Jamin (*Comptes Rendus*, Tom. XLIII. p. 1191) if μ_0, μ_t denote the indices of refraction of water at $0^{\circ}C$ and $t^{\circ}C$ respectively

$$\mu_t = \mu_0 - 0{\cdot}000012573t - (0{\cdot}000001929)\, t^2,$$

up to 30°. With the aid of this formula, which yields results agreeing closely with those obtained by Dale and Gladstone (*Phil. Trans.* for 1858, p. 889), the indices of refraction of water observed at various different temperatures may be reduced to their value at 0°.

Of the observations by Fraunhofer, (*Denkschriften der k. Akademie zu München*, B. V., S. 223, 225), the third and fourth at 8° R and $9\frac{1}{4}^{\circ}$ R respectively, are the indices of a ray which he denotes by N, coincident with the double line D of the solar spectrum. The observations by Powell (*The Undulatory Theory*

as applied to the Dispersion of Light), and by Dale and Gladstone, were made with the instrument described in the *Proceedings of the British Association* for 1839. It differs from the instruments usually employed in researches of this kind in having the circle vertical instead of being horizontal. This arrangement interferes with the possibility of measuring a double deviation, and renders the telescope liable to flexure; the absolute results are consequently less deserving of confidence than those which are comparative. Van der Willigen's observations are given in Poggendorff's *Annalen*, B. 122, S. 191.

Observer.	Temp. C.	Index for D at $t^0 C$.	Index at $0^0 C$.
Fraunhofer	18·75	1·333577	1·33449
Fraunhofer	18·75	1·333577	1·33449
Fraunhofer	10	1·33358	1·33390
Fraunhofer	11·56	1·33359	1·33399
Powell	15·8	1·3343	1·3350
Dale and Gladstone	0	1·3330	1·3330
Van der Willigen	16·58	1·33332	1·33406
Van der Willigen	22·37	1·33282	1·33400

Mean 1·33412.

But the index for D out of ice into water is 1·01952. Hence the index for D out of air into ice is 1·30858. The value of this index found by Dale and Gladstone is 1·3089. Fraunhofer estimates the place of the brightest part of the solar spectrum to be at a distance from D equal to one-third or one-fourth of the distance between the lines D, E. Taking this distance equal to (0 29) $DE = (0·062) DH$, and observing that according to Dale and Gladstone the index of refraction of water at 0^0 for the line H is 0·0108 greater than for the line D, the probable index of refraction of ice for the brightest part of the solar spectrum will be 1·30925. The value 1·3089 of the index of ice for the line D gives for the index of the brightest part of the spectrum 1·3096. These values agree

so closely with the value 1·3095 obtained by Bravais as to leave no room to doubt the adequacy of the received hypothesis. Its truth has been still further confirmed by the occurrence of a solar halo formed on the ground. (Listing, " Ueber einen in Russland von Herrn Korsakoff beobachteten terrestrischen Sonnen halo, nebst bemerkungen über das krystallisirte Wasser." Poggendorff's *Annalen*, B. 122, S. 161.) M. Korsakoff found the radius of the middle of the red ring equal to $22° 15'$ by measurement with a sextant, and traced the light to small prisms of ice scattered on the ground. When examined under a microscope these were found for the most part to be hexagonal prisms terminated at one end by a plane normal to the axis of the prism, and at the other end by a hexagonal pyramid. They were from 0·38 to 0·66 millimètres long, and from 0·13 to 0·19 millimètres thick. Crystals of ice exhibiting combinations of hemihedral forms with inclined faces had been approximately measured and described by N. A. E. Nordenskiöld, (Poggendorff's *Annalen*, B. 114, S. 662). They were combinations of the rhombohedral forms

$$1\ 1\ 1,\ 0\ 1\ \bar{1},\ \kappa\ 2\ 3\ 1,\ \kappa\ 1\ 2\ 0,\ \kappa\ 1\ 5\ \bar{3}.$$

The angles were

$$1\ 1\ 1,\ 0\ 1\ \bar{1} = 90°;\ 1\ 1\ 1,\ 2\ 3\ 1 = 38° 57';\ 1\ 1\ 1,\ 1\ 2\ 0 = 58° 14';$$
$$1\ 1\ 1,\ 1\ 5\ \bar{3} = 81° 31'.$$

Another instance in which the formation of a halo was distinctly traced to the refraction of the light of the sun by an assemblage of crystals of snow is recorded in the first *Jahrbuch des Schweizer Alpenclub.* On the 3rd of January, 1864, Professor Ritz saw a halo in the cloud of loose snow stirred up by a violent wind at the entrance of the upper valley of Kandersteg.

This view of the formation of halos is supported by the observations of Arago and Sir David Brewster, who ascertained that the light of halos was polarized in the direction of a tangent to the arc at the point observed. Bravais says that

the evidences of tangential polarization are frequently more or less masked, especially near the ends of a horizontal diameter, by the atmospheric polarization the direction of which is radial. The polarization of a halo of 22° observed by myself at Rosenlaui, in August last, was very distinct, and did not appear to be sensibly enfeebled at any part of the circle by the polarization of the atmosphere. This was probably due to the diminished amount of atmosphere above the place of observation, the height of which was about 1340 mètres above the level of the sea. The polariscope employed was Savart's. On account of the small breadth of the ring of light it was difficult to determine the plane of polarization by noting whether a dark or a light band occupied the middle place in the system of bands. But by holding a piece of varnished paper at a proper inclination, so that when viewed through the polariscope it appeared to be in contact with the arc of the halo, the dark bands parallel to the radius of the halo produced by the light reflected from the paper, coincided with the light bands seen in the halo. Consequently, the light from the paper being polarized by reflexion in the direction of a radius of the halo, the light of the latter was polarized in the direction of a tangent to the arc, i.e. was polarized by refraction.

PROFESSOR CHALLIS stated that the explanations of the phenomena of halos were simple, and ought to be included in the University course of Mathematical study; he also adverted to the importance of the instance of the halo formed on the ground.

MR POTTER (Queens') gave a brief sketch of the history of the observation of prisms of ice. He mentioned that in the course of a voyage from New York a halo 22° was observed in a cloudless sky, which was followed by stormy weather.

Further Experiments on the Synthesis of Organic Acids.
By A. R. CATTON, *M.A., F.R.S.E., St John's College.*

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The following new Fellows were elected:

J. V. DURELL, M.A., *St John's College.*
G. W. WELDON, M.A., *Trinity College.*
S. W. MITCHELL, B.A., *Caius College.*

Abstract.

The Laws which have determined the distribution of Life and of Rocks. By Mr H. G. SEELEY, F.G.S.

IN the *first* part of this paper the author, having assumed that the Earth had cooled from Igneous fusion, discussed the nature of faults, earthquakes, changes in the level of land, &c.

The *second* part was an investigation into the influence of water in distributing and modifying the relations of the material of stratified rocks, so discovering principles of their necessary sequence in space and in time.

The *third* part discussed the possibility of modification and development of the groups of life called species, &c. by the influence direct and indirect of the Sun's energy, accumulated in the organism as work ; and the influence of this force on the distribution of life.

The *fourth* part consisted of an investigation of the effect produced by the forces discussed in Part I. on the distribution of rocks and of life arrived at in Parts II. and III., whereby the author explained the present distribution of life on the Earth.

Abstract.

On the Potton Sands. *By* Mr. H. G. SEELEY, F.G.S.

THE author described the English deposits between the Kimmeridge clay and the Upper Greensand and Gault; especially as seen in the sections from Hunstanton to Oxford and Farringdon. The author proposed to restrict the name "Carstone" to the district where Sands are the only deposit from Kimmeridge clay to the Red Rock of the Wolds. He proposed that by Shanklin (or Lower Green) sand, as it is commonly called, should be understood the deposit between the Wealden and the Gault. But by "Potton sands" he proposed to distinguish the strata which occur as sands between the Gault and the Kimmeridge clay. The author thought that they represented the whole series of strata from Portland sand upwards, and compared them with the German series described by Dr Oppel.

The deposits are separated into Upper, Middle and Lower, the latter two being very fossiliferous. The fossils resemble in many ways those of the Farringdon sponge-gravels.

The origin of the rock-material and of the fossils were discussed in detail by the methods of the previous paper.

November 26, 1866.

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The following new Fellow was elected:

T. GWATKIN, M.A., *St John's College.*

The comparatively late date of our Homeric Texts.

THIS paper was an attempt to show, that although the characters and events of the Trojan war are undoubtedly of great

antiquity, the composition of the *Iliad* and the *Odyssey*, as we now have them, may probably be referred to a period even subsequent to the age of Herodotus. The author argued, from an examination of the numerous passages in Pindar that refer to the Trojan war, that that poet could not have known our *Iliad*, but that other ancient Epics took their place in his time, the same, in fact, as the Greek Tragic writers used for the themes of their plays. Out of about fifty passages in Pindar, and even more than that number of Greek Plays, the titles of which, and to some extent the subjects, are known to us, the writer contended that only two, the *Rhesus* and the *Cyclops*, could be referred to our texts of Homer ; while out of at least fifty passages in Plato, where Homer is mentioned by name, and citations made from him, there are none which do *not* refer to our present texts. Hence it was argued, that the compilation of our Homer from those older epics was made between B.C. 450 and 400; that its preservation was due to the fact that it was from the first a *written* work, and that it finally superseded the more ancient epics, which were recited by the rhapsodists, from the superior merit of the poetry as well as from its being better adapted to the advanced literature of the period when it was written. The more comprehensive poems on the *Troica,* which are commonly held to be later in date, secondary in importance, and merely supplementary to the *Iliad* and the *Odyssey*, were shown to be in all probability the original poems, reduced to writing probably after the composition of the *Iliad* and *Odyssey*, but wholly unable to compete with them in merit, and therefore regarded in much later ages, in which we first find any separate account of them, as the inferior productions of post-Homeric poets. The author further argued from the remarkable resemblances between the style and diction of Homer and Herodotus, and from the generally Asiatic character of the Homeric similes and scenery, that the compiler of our poems was an inhabitant of Asia Minor, and that possibly the poet Antimachus of Colo-

phon, who is recorded to have been an "editor" of Homer in the age of Pericles, may have had more to do with the actual authorship than is commonly thought. This theory was shown to be supported by the very modern style of a great deal of our Homeric Greek, combined, as might be expected, with archaic forms retained from the earlier epics. Evidence to the same effect was adduced from the early Greek Vases.

The PUBLIC ORATOR (Rev. W. G. Clark, Trinity) stated that Hipparchus was said to have introduced the Homeric poems into Greece, and to have had them committed to writing; that it was very improbable that a work of such magnitude should have been introduced without detection at an age such as that assigned by Mr Paley as its probable date. The impression produced upon his mind by reading the *Iliad* and *Odyssey* (which he had done on the spot) was that they were poems belonging either to different ages or to different states of society. He accounted for the occurrence of modern forms in the poems by supposing that they might have crept in from time to time until the text became fixed. Writing had been in use in Egypt at a very early date; for instance, there was a Hieratic papyrus in the British Museum to which the date 1100 B.C. was assigned. Good subjects for Tragedies were not to be found in the Homeric poems, and this was sufficient to account for the rarity of Tragedies founded on them.

MR PALEY replied that he did not suppose that the Homeric poems mentioned in connexion with Hipparchus were those which we possess; and that there were no words for 'reading' and 'writing' in early Greek, so that although they might be known in Egypt, they were not in Greece.

NOTICE.

AT a Meeting of the Council of the CAMBRIDGE PHILOSOPHICAL
SOCIETY, *Nov.* 12, 1866, the following Resolution was
carried :

"That every Fellow of the Society, who has paid his sub-
cription for the year, or the composition in lieu of annual
ayments, be entitled to receive a copy of every Part of the
Transactions published during the year to which his subscription
efers."

Any Member of the Society can have his Part on personal
pplication, or on sending an order (with stamps for postage,
f necessary) to one of the Secretaries.

*** *Part I. Vol. XI. is now ready.* (*Postage 6d.*) *The
ndex to Vol. X. will be published with the next Part.*

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

February 18, 1867.

The President (H. W. Cookson, D.D., Master of *St Peter's College*) in the Chair.

The following new Fellow was elected:

Rev. Joseph Castley, M.A., *Pembroke College.*

On the continued change in an Eye affected with a peculiar malformation. By George Biddell Airy, M.A., LL.D., D.C.L.; *formerly Fellow of Trinity College; and late Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge; Astronomer Royal.*

On two occasions, of which the first occurred forty-one years ago, I have communicated to the Cambridge Philosophical Society the description and numerical elements of a malformation affecting my left eye. In the first communication, dated 1825, February 5, I gave as the characteristic of the malformation this result of experiment; that, on viewing a very small luminous point, formed by the light passing through an extremely minute hole in a blackened card, the luminous point, at one distance, presented to the malformed eye the appearance of a sharply defined line nearly horizontal, and, at a greater distance, presented the appearance of a sharply defined line at right angles to the former line, and therefore nearly vertical. And, as numerical elements, I gave the measures of the

5—2

two distances of the luminous point from the front of the cornea (with sufficient accuracy) when these two appearances presented themselves; the card being made to slide upon a graduated scale whose end rested against the orbital bone of the eye, and the eye view being directed steadily forward. I may here mention that the method of experimenting in the subsequent observations has been precisely the same.

The second communication was dated 1846, January 14, or more than twenty-one years after the first. In this, besides giving the elements of astigmatism of the left eye, in the same manner in which I had given them in 1825, I gave the measure of the focal length of the right eye, which is perfectly stigmatic.

After another period of more than twenty years, I am now enabled to give similar measures for both eyes.

The comparison of the similar elements at the three different epochs may perhaps be interesting.

I. Distance from the cornea of the left eye at which the luminous point presents the appearance of a nearly horizontal line.

In 1825, 3·5 inches; Reciprocal = ·286. Difference − ·073.
In 1846, 4·7 ... ; ... ·213.
In 1866, 5·4 ... ; ... ·185.   ... − ·028.

II. Distance from the cornea of the left eye at which the luminous point presents the appearance of a nearly vertical line.

In 1825, 6·0 inches; Reciprocal ·166. Difference − ·054.
In 1846, 8·9 ... ; ... ·112.
In 1866, 10·6 ... ; ... ·094.   ... − ·018.

III. Measure of the astigmatic power of the left eye at different epochs; estimated in each case by the differences of the reciprocals in the last table.

In 1825, ·120.
In 1846, ·101.   Difference − ·019.
In 1866, ·091.   ··   − ·010.

IV. Focal length of the right or stigmatic eye, measured from the front of the cornea.

In 1846, 4·7 inches; Reciprocal ·213.
In 1866, 5·5 ... ;   ...   ·182.   Difference − ·031.

The following conclusions appear to be supported by the examination of these numbers.

1. The change in both focal elements of the left eye is far greater between early manhood and full manhood than between full manhood and approaching age; and the change in the astigmatic power is decidedly greater between early manhood and full manhood than between full manhood and approaching age.

2. The most constant of the elements is the astigmatism.

3. Through the period 1846—1866, the distance for the left eye corresponding to the appearance of a nearly horizontal line is sensibly the same as the focal length of the right eye.

G. B. AIRY.

Royal Observatory, Greenwich,
      1866, *November* 19.

On some points in the Anatomy of the Chimpanzee, and the consideration of the term 'Quadrumanous,' as applied to that animal. By Professor Humphry, F.R.S.

His remarks were the results of the recent dissection of two Chimpanzees, and referred chiefly to the differences between their lower limbs and those of Man. He pointed out that the

outer condyle of the thigh bone is round instead of being pro-
longed from before backwards, and flattened beneath, as in Man.
Hence there is comparatively little security afforded by the
ligaments in the straight position, and little provision for the
maintenance of the erect posture. The bones and joints of the
ankle were shown to be constructed so as to permit free move-
ment rather than to bear weight. With regard to the term
"hand," and the objections which have been urged to its appli-
cation to the lower limb of the monkey, Professor Humphry
remarked that if we use the term to designate a certain modifi-
cation of the fore limb—a certain deviation, that is, from the
ordinary fore foot—we may with equal propriety apply it to a
corresponding modification of the hind limb—a corresponding
deviation, that is, from the ordinary hind foot. We must not
expect it closely to resemble the human hand, but merely to
present such a similarity to it as the special features of a hand,
viz. the shortness, mobility and opposeableness of the thumb and
the relative length of the other digits, would give it. Judging by
this rule, it is as correct to speak of the "hind hand" of a monkey
as of the "fore hand," though, forasmuch as both are employed in
progression, it may, perhaps, be better to use some other term,
such as "cheiropod" for the designation of the class, leaving
the term "bimanous" to indicate the characteristic feature of
man. The psychical qualities should not be omitted in con-
sidering the distinctive features of man; and the importance of
the long, strong, firm great toe in this respect was pointed out.
Some other peculiarities in the limbs and in the prostate gland
of the Chimpanzee were described.

March 4, 1867.

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

On Roslyn or Roswell Hill Clay-pit, near Ely. By the REV. O. FISHER, M.A., F.G.S.

ROSLYN or Roswell Hill Clay-pit has long been a standing puzzle to Cambridge geologists. I have visited it several times, and have notes upon it made in 1853 and 1856. I was there in November of the present year, having by Professor Sedgwick's permission the assistance of H. Keeping.

The pit is probably well known to you. It covers several acres of ground, and extends in a direction N.W. and S.E. The material has been used for the purpose of making up the banks in the fens, and the section is comparable to that of many natural cliffs.

The northern side of the pit is occupied by horizontal Kimmeridge clay, which is or used to be capped here and there by a thin covering of lower green sand. At the western end of the pit boulder clay of a typical character abuts against the Kimmeridge clay, the plane of junction running nearly east and west, and dipping at a high angle under the boulder clay. The southern side of the pit, as at present exhibited, shows a blueish grey cretaceous clay, flanked at either end by nearly vertical chalk marl, which becomes somewhat argillaceous towards the eastern end of the pit. The chalk marl and clay are evidently in true sequence.

The question which I propose to discuss relates to the singular collocation of these several beds.

I will first of all consider the presence of the chalk marl and the clay on the northern side of the pit.

I have said they are clearly in sequence. Their junction is perfectly natural. The chalk marl becomes sandy, and contains

a few scattered nodules of phosphate of lime, with some of the fossils of the upper green sand usual in the neighbourhood of Cambridge, and then the clay succeeds. The character of the beds here has been so well described by Mr Seeley[1], that I need not say more about them, except that I think it open to question whether the clay is really gault. There is about Cambridge a band of clay in the lower part of the chalk, which was well shown in the construction of the waterworks at Cherryhinton, and I am rather inclined to think that the Ely clay belongs to the same bed. I recollect phosphatic nodules occurring in connection with it at Cherryhinton. I think the abundance of shells, and especially of Perna, in the Ely clay, rather militates against its being gault. But I merely throw out this as a suggestion[2].

Boring by Mr Docwra at Cherryhinton waterworks noted in 1854.

	ft.	in.
"Soil	1	0
Plastic	29	0
Upper green sand with fossils	0	10
Clunch	6	0
Sand	0	8
Gault not pierced	45	0
	82	6"

I visited the spot and found belemnites in the "Plastic," and coprolites from the supposed upper green sand. From a subsequent cutting made to convey water from near Mr Okes's house, I recollect observing that the above-mentioned "plastic" was a stratum in the clunch. This section makes a coprolite layer beneath the clay. There may also be one above it.

[1] *Geol. Mag.* Vol. II. p. 529.

[2] At a subsequent visit, 26 Apr. 1867, I saw the lower green sand in sequence to this clay, which would make it the true gault. In another part of the pit the gault reposed on boulder clay with chalk pebbles.

At any rate we may look at the chalk and clay near Ely as a single mass, and whatever accounts for the presence of one will equally account for the other. In short they are a large mass of cretaceous beds in a nearly vertical position, with boulder clay abutting upon them. The curved lines of junction as seen in the section are nothing more than the curves formed by the intersection of the surface of the workings with a nearly plane surface of junction between the chalk and the gault, dipping at a very high angle towards the north.

Now there are two ways of accounting for the presence of this cretaceous mass. It is either brought *up* by a fault with reference to the boulder clay, but *down* with reference to the Kimmeridge, or else it is a huge boulder, forming as much an integral part of the boulder clay as any block of oolite or flint which it contains.

Mr Seeley appears to consider its presence best accounted for by a fault, but I think I shall be able to show that the other is the more probable explanation.

And to clear away any possible *a priori* objection drawn from the magnitude of the mass, I would beg to remind you that chalk boulders occur in the Norfolk drift so large that quarries and limekilns are worked in them.

Extract from a letter by the Rev. John Gunn :—

<div style="text-align: right">

IRSTEAD,
Dec. 10, 1866.

</div>

" Of the masses of chalk you enquire about, that near Castle Rising is now exhausted and used for top-dressing land. Only the large flints remain to prove that the mass belonged to the upper chalk which does not remain anywhere in that part of West Norfolk.

" The largest detached mass I know of is between Cromer and Overstrand. I do not know the precise boundary of the

parishes. It has been for years used for lime and a kiln is on the premises. Mr Prestwich as well as myself noticed a layer of sand beneath it.

(*Obs.* They evidently looked for proof that the mass was not *in situ,* showing how nearly it simulated a natural bed of chalk.)

" In several places north of Cromer from that place to Sherringham are large masses of bouldered chalk proved to be so by the underlying beds.

" On the south side also at Barton and Happisburgh there were some but they have all been washed away. In North Walsham, Worstead, and Witton large bouldered masses have from time to time been worked either for making lime or for top-dressing. A tooth of *Elephas prim.* was obtained by me at Witton in connection with one.

" The large masses at Trimmingham figured in Lyell's elements are part of the fundamental chalk, remnants of an upper bed from which the gravel of East Norfolk is derived."

These instances show that the mere size of the mass of cretaceous strata at Ely is no argument against its having been carried thither by ice, and the fact of its consisting of portions of two distinct beds is a mere accident.

There is nothing singular in so large a block of chalk becoming detached from its parent bed. For some miles along the coast west of Lyme Regis landslips on a large scale have occurred where masses of chalk and green sand fully equalling in bulk the mass at Ely have fallen from the cliff. The last of these falls occurred not many years ago. The lower portion of the disengaged strata consisted of a sandy loam, the upper of chalk.

If we could conceive such circumstances under a glacial climate that this mass could have been floated away, as for instance by snow blowing over the top of the cliff and being frozen on to its face, we should have all the conditions necessary for the deposition of an immense boulder like that at Ely;

and on this supposition we might expect it to have been dropped in a similar position of verticality, for the float would have been attached to its edge. But without making such a supposition, knowing how frequently icebergs roll over in the process of thawing, we may expect them to drop their loads indifferently in all positions.

I was originally disposed to think this mass a boulder, when I saw it ten years ago. It was then much less exposed than it is at present. I was quite confirmed in that opinion by what I saw the other day[1].

The mass seems to be much of the shape of a great punt, the prow of which is directed towards the west. The mass is so thick towards the stern, *i.e.* at the eastern end, that they have not dug through it, but towards the centre the workmen told me they found a " snuff-coloured hard clay" at the bottom of the pit beneath the chalk, " hard clay" being the term by which they designate the boulder clay. Towards the western end of the exposure of the chalk this clay may be seen beneath it in the section, enclosing angular lumps of chalk. Towards the cottage on the bank the chalk thins out to nothing, the boulder clay passing beneath it.

I believe the lower green sand blocks which occur on the north side hereabouts to be no more *in situ* than the chalk.

The boulder clay between the chalk and Kimmeridge clay shows tortuous streaks of bedding, some of them chalky, in a highly inclined position. They seem to have been originally horizontal as well as the chalk. But no one who has seen the Cromer cliffs will think any mode of bedding too strange to occur in the boulder clay, or call in the aid of a fault to account for it.

The second part of the enquiry relates to the occurrence of this mass of boulder clay in juxtaposition to the Kimmeridge. The question lies between a fault and a great channel of erosion, made for itself by the glacial drift.

The only evidence upon this point to be obtained *in the pit*

[1] See also note 2, p. 52.

is by examining the junction. If the country were mapped, and a fault affecting other strata traced through the pit, this would settle the question in favour of a fault. There seem to be disturbances in the neighbourhood. We have Oxford clay, for instance[1], at the bottom of the hill near the railway station, where Kimmeridge clay would have been more natural. And other places might be named (Aldreth and Alderforth). But that faults affect the oolites affords only a slight presumption that they will also affect the boulder clay. With regard to the evidence to be obtained at the spot itself, I first of all attempted to examine the junction by digging in the side of the pit; but I found that, owing to a line of springs thrown out by it, the boulder clay has slipped, so that I could not reach the undisturbed ground. This circumstance misled me when I examined the place in 1856, and made me suppose the junction showed slickenslide, which was really due only to a recent slip. I then searched for and found the junction in one of the banks left by the workmen to exclude the water as they dig.

Here I found it well defined, but I could not discover any of those symptoms of pressure, or the polished surfaces, which are always observed to accompany a fault. As far then as the evidence goes it is against the occurrence of a fault, and points to the boulder clay occupying a trough, which it has ploughed out for itself in the old sea bottom of Kimmeridge clay. Such troughs I believe to be not uncommon in districts bordering upon extensive spreads of the boulder clay.
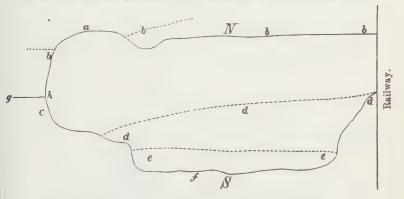
I have made diagrams of sections seen in two boulder clay pits at Gillingham in Norfolk, and at Bulchamp in Suffolk, which illustrate the manner in which the sea bottom has been eroded by icebergs, and the cavities filled with boulder clay. In the instance at Bulchamp, which I saw with Professor Liveing last summer, the sea bottom has consisted of a sand,

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[1] I have since learned, however, that a well at Ely commenced in the Kimmeridge soon reached the Oxford clay with a thin stony band containing *Nerinœa* intervening.

of an age one degree anterior to the boulder clay. This case
has been, like that at Ely, adduced as an instance of faulting;
but we noticed sand of the same character as that at the side of
the section, clearly continued beneath the clay.

In the other case the boulder clay has been originally depo-
sited upon the same sand, but has been subsequently itself
eroded down to its very base, and the channel filled again by a
fresh deposit of slightly different materials.

Ground Plan of the Ely Clay-Pit. The width from N. to S. is exaggerated.

(*a*) Lower green sand. (*b*) Kimmeridge clay.
(*c*) Erratic clay, with boulders of granite, oolite, large flints, &c.
(*d*) Chalk. (*e*) Gault (?)
(*f*) Lower green sand. (*gh*) Line of junction.
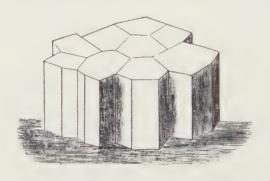
Note on a Case of Prismatic Structure in Ice.
By T. G. Bonney, B.D., F.G.S.

On Jan. 26, 1867, the attention of the writer was attracted,
while walking with a friend in the Fellows' Garden of Christ's
College, by the appearance of the ice on a pond. On pro-
ceeding to examine a fragment from near the edge, he says,
"it was about ¼ inch thick, and was a mass of prisms with
their axes perpendicular to the surface. The ends of these
in one part were very irregular polygons; and the lines join-

ing opposite angles, speaking roughly, were on an average about one inch in length; but in another part the forms appeared rather more regular—hexagons being common—the diameters of which were about ¼ inch. Another fragment was then obtained rather further from the edge; this was a little more than one inch thick, and consisted of prisms whose ends were about the same size as those just described. Though the number of sides in their polygonal ends was not constant, six was certainly a common number, and this appeared to be rarely exceeded; the angular points of the polygons were a little blunted, so that the sides were slightly curved. The angles were thus difficult to estimate, and I had no instrument, but I do not think that they were constant. The ice broke

very easily along the sides of these prisms, and never through or athwart one of them; so that each fragment had a beautifully dentate edge (see Fig.), and its side resembled a miniature group of basaltiform columns. Single columns could be easily detached quite perfect, with the point of a penknife, or even with the finger-nail. The ice contained a few air-bubbles and a chance bit of weed or bark here and there, but was in no other way remarkable. We carried a large piece of this last described to my rooms; and after hastily improvising a freezing bath of snow and salt, subjected some fragments of it to a temperature of about − 5° Cent. (for a short time the thermo-

meter sank $3°$ or $4°$ lower, but this was the general temperature). After leaving it here for some time we examined it carefully; the prismatic structure was entirely obliterated, the only traces of it being the slight surface depressions here and there which marked the edges of the prisms, and certain vertical chains of small air-bubbles which had formed in the interstices at their angles. The ice was very hard, and, when broken, exhibited the usual conchoidal fracture. I was anxious to see whether the prismatic structure would return as the ice thawed again, but unfortunately the fragment laid aside for this purpose got in contact with some of the melted salt and snow of the bath, and was dissolved too rapidly."

The author then expresses his opinion, derived from the examination of this case and of the ice in some of the glacières of Savoy, that the axes of the prisms are always perpendicular to the ice-surface, whatever its form may be; that the structure is only developed under peculiar conditions, viz. when thawing takes place slowly and without disturbance in a place 'sheltered from wind and sun. The following table, from the register kept by Mr Pain, Sidney Street, shews the temperature in the neighbourhood during the days of thaw preceding Jan. 26.

	Jan. 22.	Jan. 23.	Jan. 24.	Jan. 25.	Jan. 26.
At 9 A.M.	28·2	44·5	51·5	46	40
During past 24th.					
maximum ...	30	44	53	55	50
minimum ...	26	22	43·5	42	34

March 18, 1867.

G. E. PAGET, M.D., VICE-PRESIDENT, in the Chair.

The following new Fellows were elected:

G. H. EVANS, M.A., *King's College.*

E. CARVER, M.A., *St John's College.*

J. B. BRADBURY, B.A. and M.B., *Downing College.*

R. K. MILLER, B.A., *St Peter's College.*

J. E. FINCH, B.A., *Trinity Hall.*

J. F. WALKER, B.A., *Sidney Sussex College.*

B. W. EARLE, B.A., *Jesus College.*

W. K. CLIFFORD, B.A., *Trinity College.*

On the difference of Longitude between the Society's Clock and the transit Clock of the Cambridge Observatory. By PROFESSOR CHALLIS, F.R.S.

THE author described the instrument used and the means adopted in determining the difference, and stated that it was 5·88″, and that the longitude of the Society's clock was 28·52″ east from Greenwich.

On the Meteoric shower of November, 1866. By PROFESSOR ADAMS, F.R.S.

THE author described the instrument used in the observation of the Meteors, and mentioned the various hypotheses which have been advanced concerning the orbit of these bodies; he explained the calculations which he had made to determine this, and shewed that the attractions of the Earth, Jupiter, Saturn and Uranus were nearly sufficient to account for a hitherto unexplained change of about 29 minutes in the position of the nodes of the orbit in each period of 33 years. He called attention to the fact that the orbit calculated appeared to coincide very nearly with those of certain comets; and held that the latter were elongated ellipses with a periodic time of 33 years.

PROFESSOR CHALLIS and PROFESSOR MILLER made some remarks on this communication, in which the former expressed himself not quite convinced by the arguments of PROFESSOR ADAMS.

Erratum.

To title of Second Paper on page 40, add 'by F. PALEY, M.A., *St John's College.*'

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

April 29, 1867.

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The following new Fellow was elected:

T. W. DUNN, B.A., *St Peter's College.*

IT was announced to the meeting that the Hopkins Prize was adjudged to Professor G. G. Stokes, Sec. R.S., for numerous memoirs on various questions in Pure Mathematics and Mathematico-physics; for his discovery of the change of the refrangibility of light and the application of spectrum analysis in Opticochemical investigations; and particularly for his Paper on the long spectrum of electric light published in the *Philosophical Transactions* for 1862, Part II., printed and circulated in 1863.

THE FOLLOWING ARE THE REGULATIONS FOR THE "HOPKINS PRIZE."

I.   THAT the Prize be called "THE HOPKINS PRIZE."

II. That this prize be adjudged once in three years.

III. That it be adjudged for the best original memoir, invention or discovery, in connexion with Mathematico-physical or Mathematico-experimental science that may have been published during the three years immediately preceding, but that the adjudicators be at liberty, if it seem to them advisable in any particular case, to award the Prize for a discovery in Mathematics alone, or in Experimental Physics alone, or for one which has not been published within the fore-mentioned period.

IV. That it be confined to those who are or have been Members of the University of Cambridge.

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V. That the fund be vested in the Cambridge Philosophical Society, and the prize adjudged by three Fellows of that Society, nominated by the Council of the Society for each occasion.

VI. That, in the event of any difficulty arising in carrying out the above provisions in any particular instance, either from lack of a prize-subject of sufficient merit, or from any other cause, the Council of the Cambridge Philosophical Society be at liberty to carry over the amount of the Prize for that term towards augmenting the fund for future prizes, or to award it to some one not a Member of the University.

On Capillary Attraction. By Mr POTTER.

May 13, 1867.

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The Treasurer made his financial statement, his accounts were passed, and the thanks of the Society were returned to him.

On Modern Musical Scales. By HARVEY GOODWIN, D.D., *Dean of Ely.*

THERE are two points connected with the system of musical scales universally adopted in Europe in modern times, upon which I have long desired to have clearer notions than I have been able to gain from books.

I. The first is the principle upon which we pass from one key to another by the introduction of a new sharp or flat into the signature.

II. The second is the reason why the division of the notes in the ordinary diatonic scale, artificial as it manifestly is, has become the universal division of European music, and appears so simple and natural.

Upon these two points I propose to offer some suggestions in this paper, premising to professional musicians (if the paper should fall under the eye of such) that musically I write only as an amateur. This, however, is perhaps of no great importance, as the question is one rather of numbers and mathematics than of technical musical knowledge.

I. It is well known to every one acquainted with music that the ordinary musical keys, having for their signatures respectively *no* sharps, *one* sharp, *two* sharps, &c. are formed each from the other by sharpening the subdominant of the scale, and so bringing it within half a tone of the dominant to which in the next scale it becomes the leading note. Thus we pass simply from any key to the key of the dominant, and the transition is so easy that even in the least complicated compositions, as for example in hymn-tunes, the modulation constantly takes place. The tonics or key-notes in the scales thus formed are C, G, D, A, E, B.

Now it is not very easy to see the manner in which these successive scales are related to each other, nor why the system is so complete as it appears to be. But the relation may be exhibited to the eye, and the symmetry of the system consequently made plain, in the following manner.

Let us regard the semitone intervals of the chromatic scale as being all equal, which, though not true upon any theoretical principle, is true according to that system of temperament, upon which pianofortes have long been tuned and upon which (as I understand upon good authority) it is now becoming the practice to tune organs. Then the musical interval between each note and its octave will be divided into twelve equal intervals, and these intervals may be conveniently represented
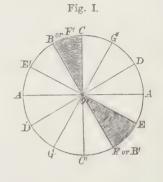
by angular spaces of 30° each, the whole twelve thus amounting to 360°, and so representing what may be regarded as the actual coincidence to the ear of the tonic and its octave.

The meaning of this will be seen from the annexed figure.

With centre O describe a circle, and divide the circum-ference into twelve equal parts. Join the dividing points with the centre, and put letters as in the figure.

Fig. I.

It will be observed that each diameter has the same letter at each extremity, the letters at the two extremities being distinguished by an ac-cent affixed to one of them; it will also be observed that one diameter has an ambiguity,
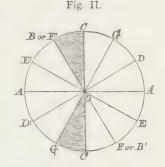
being either the diameter BOB', or the diameter FOF'; this arises from the fact that in the natural diatonic scale F is only a semitone removed from E, and B only a semitone from C. In the figure the angular spaces BOC, EOF have been shaded, to indicate at once to the eye that these are the two semitone intervals in the diatonic scale.

For the sake of distinctness, call COC' the *tonic line*, and GOG' the *dominant line*. Then it will be seen that the tonic line and the dominant line are inclined to each other at an angle of 30°: they are in fact next to each other in the group of note-lines which have been drawn through O.

It will be seen also that the shaded spaces corresponding to the two semitones are situated symmetrically with respect to DOD', the line corresponding to the second note of the scale.

Now let us see what effect will be produced upon our figure by sharpening the subdominant F. The arrangement will then

be as in Fig. II. Comparing Fig. II. with Fig. I. it will be seen that the shaded spaces of Fig. II. are symmetrical with respect to AOA', as those of Fig. I. were with respect to DOD'; hence A is the second note of the new scale, in other words the key-note is G. For it will be observed that Fig. II.

Fig. II.

represents a scale of precisely the same kind as Fig. I.: in each case the shaded portions occupy one side of a diameter, leaving one semicircular space wholly unshaded, and containing between them an unshaded section of 120°. The eye will in fact at once perceive that if we start from G in Fig. II. and pass round the circumference of the circle in the sense of the motion of the hands of a clock, we shall come to exactly the same succession of tones and semitones as we should in Fig. I. if we started from C.

GOG', then, has now become the *tonic line;* but this line, regarded merely as to its direction, (I mean, GOG' being regarded as the same line as $G'OG$,) is removed only one division, or 30° degrees, from the original tonic line. Hence it may be said, that the effect of sharpening the subdominant is to turn the tonic line through one semitonal angular space, or through 30°; and as the sharpening of the subdominant of the key of C has brought us to the key of G, and turned the tonic line into the position GOG', so the sharpening of the subdominant of the key of G will turn the tonic line through another angle of 30° into the next position DOD', or will bring us to the key of D, and so on.

Hence if we drop the accents, the figures above drawn will give us, by looking at the extremities of the successive radii, the key-notes of the consecutive major scales, namely,
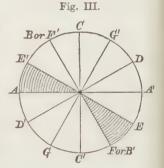
C, G, D, A, E, B.

After this there is an apparent discontinuity, but not a real one, as the next key would be that of F♯, and this is in fact the key indicated by the line OC', being intermediate to OF and OG.

Thus the passage from one natural key to another is represented by the orderly revolution of a radius of our musical circle through angular spaces of 30°.

It is hardly necessary to say that the same method is applicable to the representation of the succession of flat keys : but it may be interesting to exhibit the method to the eye.

The successive flat keys are produced by flattening the leading note, or seventh of the scale. If we perform this process upon Fig. I. we have the annexed figure. Here the shaded semitonal spaces are symmetrical with respect to GOG' exactly as in Fig. I. they were symmetrical with respect to DOD', and in Fig. II. with respect to AOA'. Consequently G is the second note of the scale, or F is the tonic.

Fig. III.

Hence for the flat keys our tonic line revolves through one angular space of 30° in the sense contrary to the motion of the hands of a clock, or contrary to that in which it revolves for the sharp keys; and therefore Fig. III. will give us for the key-notes corresponding to the signatures one, two, three, four, five flats, the following

$$F, \ E', \ A', \ D', \ G',$$

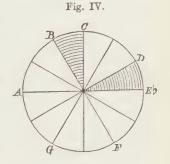or, as will be seen by inspecting the figures,

F, B♭, E♭, A♭, D♭.

II. I now pass on to the second subject which I proposed to discuss in this paper ; and I shall endeavour to exhibit by the machinery already introduced the great convenience of the

arrangement of the notes in the diatonic scale, and to suggest grounds for believing that no other division would be equally convenient.

Let us resume Fig. I. It will be seen that the musical circle representing the diatonic scale is divided into twelve equal portions, and that these twelve equal portions are divided into two groups, one consisting of four portions, or *two* whole tones, the other consisting of six portions, or *three* whole tones, by the two shaded portions corresponding to the semitones. Hence it is obvious that the shifting of the semitones so as to take one from the larger division of *three* tones and to add it to the smaller division of *two* tones, will leave a musical circle divided exactly as before ; that is, there will still be two great divisions of *two* tones and *three* tones respectively, separated by semitones. The arrangement reproduces itself.

Not only is this the case, but it is easy to see that no other arrangement of the semitones would produce the same result. Suppose for instance we have the circle divided as in Fig. IV., that is, into two groups of *one* tone and *four* tones respectively, separated by the two semitones ; in other words, regarding C as the tonic, suppose that we have a *flat third*. Then it is manifest that by no shifting of the semitones can this arrangement be made to reproduce

Fig. IV.

itself. In fact the problem of making such a self-reproducing scale is merely that of dividing 5 into two parts, such that if unity be taken from one and added to the other the two parts shall be the same as before. It is manifest that the division into 2 and 3 is the only solution.

The division of the circle represented in Fig. IV. is some-what interesting from the fact that it is the actual division

in an ascending minor scale. The arrangements of the semi-
tones in the ascending and descending scales of a minor key
are, as every one acquainted with the elements of music knows,
different, and the signature is that which corresponds to the
descending scale, the semitones being put into their proper
places in the *ascending* scale by means of two accidentals.
The arrangement of the semitones in the descending scale, if
represented according to the method of this paper, is the ordi-
nary diatonic arrangement: for example, Fig. I. would represent
the descending scale of A minor, if we pass round the circle
from *A* in the sense opposite to the motion of the hands of
a clock; in other words the key of A minor has the same
signature as that of C major; and as the signature is thus
taken from the descending scale the modulations from one key
to another in the minor scales follow the same rule as those
in the major; but this would not be the case if the arrange-
ment of semitones were that which, because it is more pleasing
to the ear, we adopt in the ascending scale.

These considerations seem valuable with reference to the
question, What is the reason why in modern Europe the com-
mon diatonic scale has gained such universal acceptance ? It
is a mistake to suppose that it is a scale founded upon any
natural necessity ; if so, it would be universal, which is not
the case. But this seems to be the fact, namely, that there
is such a perfection in the arrangement as ensures its adoption
as soon as known, and guarantees its permanence to the exclu-
sion of all others, except so far as a different arrangement may
sometimes produce a feeling of pleasure by its novelty or its
eccentricity. Nor is it perhaps difficult to deduce the diatonic
division from simple principles, and to shew that it is not so
arbitrary as at first sight it may seem.

The first principle must be the identity of a note with its
octave. I speak of the *octave*, and by so doing appear to
anticipate the existence of eight notes in the scale ; but I do not

ntend to make this anticipation. I only use the word octave
to describe that note which is produced by vibrations of air
twice as rapid as those which produce the fundamental note.
Experience teaches us that the coincidence of two notes so
related is acoustically perfect, so that they may be regarded
as the same, and we may with propriety speak of the upper
C or the lower C, applying the same letter C to express two
notes, which, mechanically speaking, differ from each other, but
which musically may be regarded as identical.
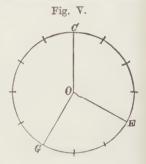
The question of the musical scale therefore resolves itself into
that of interpolating a convenient number of conveniently related
sounds between a note and its octave. Of all possible sounds
which may be interpolated there are two which seem to have
a chief claim to admission. These are the *third* and *fifth*,
according to the common nomenclature ; but mathematically
speaking they are sounds produced by vibrations bearing a
very simple numerical relation to those which produce the
fundamental note, and musically speaking they are sounds
which produce a very perfect harmony with the tonic and
octave ; and when the four notes are sounded successively, there
is a simple and majestic progress from one to the other which
every ear at once recognizes with pleasure.

So far all is tolerably simple, but the problem still remains
to interpolate notes amongst those four, which we admit without
question as the chief in the scale ; and the problem branches out
still further into the more complicated one of *temperament;* upon
this many treatises have been written, and a compendious ac-
count of the question may be found in Sir John Herschel's
Treatise on *Sound* in the *Encyclopædia Metropolitana.* It is not
the purpose of this paper to enter into this difficult subject ;
but without doing so, I think it may be shewn by reference
to the mode of illustration which has been adopted, that we are
(as it were) forced into the arrangement of tones and semitones
which constitutes the ordinary diatonic scale. For if we admit

the principle that a semitone is the smallest interval by which it is agreeable to the human ear that musical sounds should follow each other, and if we further admit that the third and fifth must find their place in the scale, then we find our musical circle divided into three portions which contain *about* (not accurately) 4, 3, and 5 semitones respectively.

No other division of these intervals seems possible, except that of dividing the first two in- tervals into two parts each, and the last into three. The positions of the two semitones, the existence of which is manifestly unavoidable, will still be undecided, but there will not be much difficulty in determining their position ; for the satisfaction which the ear experiences in the sound of a *leading note*, or a note approaching

Fig. V.

within half a note of the tonic, is so great as to leave no doubt as to the position of the semitone between *G* and *C*; it must clearly be immediately contiguous to *C*; and this being so, it will be indifferent where we place the other semitones. For suppose we put it next to *E*, then we have the ordinary arrangement of the diatonic scale, the tonic being C: but suppose we take the other course and put it next to *G*, then the result is that the arrangement of semitones in the musical circle is exactly the same as before, only the tonic will be G instead of C. Hence, granting to the *third* and *fifth* their places in the scale *honoris causâ*, and allowing the necessity of a leading note, it appears that the arrangement of the semitones in the scale *must* be that with which we are familiar. And thus we seem to get at a *rationale* of the ordinary system of notes, which is in some respects more instructive than that which is usually given, as for example in the treatise of Sir John Herschel above cited; for the reasons there adduced depend upon considerations of the

umber of harmonies which can be made amongst the various
otes of the octave or of successive octaves; and these con-
iderations are valuable; but there would seem to be a propriety
i the arrangement of the notes independent of them; there is a
ately march of sound in the ordinary gamut which is highly
atisfactory to the ear, and for which considerations of harmony
o not seem to me to account.

One more point occurs to me as worthy of notice. I have
poken of the third and fifth of the scale as claiming their
laces before all other notes. There can be no doubt of this as
egards the fifth; it fully deserves the title of the *dominant;*
hen we listen to a piece of music, its sound is left upon the ear
lmost, if not quite, as clearly as that of the tonic itself; and
nathematically speaking the numbers which denote the ratio
f its vibrations to those of the tonic are simpler than in the
ase of any other notes. But there may be a demur to the same
recedence being granted to the third, at least to the *major*
hird, because it may be argued that the succession of notes is
s pleasing and satisfying to the ear if for the major we sub-
titute a minor third; that is, if we take as the basis of our
ystem of notes the succession

<div style="text-align:center">C, E♭, G, C,</div>
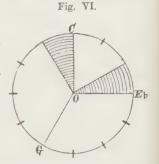
istead of

<div style="text-align:center">C, E, G, C.</div>

Let us, then, just examine the conclusions to which we shall
e led, if we start with the minor third as one of the primary
itervals.

Our musical circle will now be as in Fig. VI.; and the
uestion will be, where shall we put the semitones? In the
scending scale the demand for a leading note will lead us to
ut one of them below the upper C; and with regard to the
ther we easily perceive that the effect of the flat third is
ost unless the note preceding it be distant by a small interval
r by a semitone; not to mention that the juxtaposition of

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two semitones, which is our only alternative, would be intolerable.
Hence we arrive at the arrangement shewn in Fig. vi., which is that of the ordinary ascending minor scale; but it has been already pointed out that a scale of this kind does not admit of being changed into another, as does the diatonic arrangement; and it is fortunate that in the descending scale the ear does not by any means demand the small interval of the semi-

Fig. VI.

tone between the eighth and seventh of the scale ; on the other hand the peculiar and indescribable effect of the minor key, of which the flat third is the mainspring, is increased by dropping a whole tone instead of half from the eighth to the seventh.   Taking, therefore, the descending scale and beginning with a fall of a whole note, the position of the other semitone settles itself; for, whether we put it third in the scale or fourth, it will be seen from inspection of the figure that we get no different arrangement of the notes : in each case we shall have two groups of two and three tones respectively separated by two semitones; in other words, in each case we shall have the ordinary diatonic arrangement.

In concluding this little essay, I will express the hope that the views which have been propounded, and the method employed for their illustration, may tend to give simplicity and clearness to a branch of science, the fundamental principles of which, though in some respects easy and familiar, are in others not free from obscurity.

H. GOODWIN.

Professor CHALLIS (VICE-PRESIDENT) in the Chair.

The following were elected Honorary Members of the Society :

> MM. EHRENBERG,
> ,, PONCELET,
> ,, PLÜCKER,
> ,, AGASSIZ,
> ,, QUETELET,
> Dr DAUBENY,
> Dr TODD,
> Mr STEPHEN SMITH,
> Mr MAX MÜLLER.

(1) *On a New Method of maintaining the Oscillations of a Pendulum. By* W. H. MILLER, M.A., FOR. SEC. R.S., *Professor of Mineralogy in the University of Cambridge.*

Professor Challis expressed his admiration of Professor Miller's contrivance, and described the practical difficulties which he had experienced with ordinary pendulums at the Observatory.

(2) *On the Crystallographic Method of Grassmann, and on its employment in the investigation of the general geometric properties of Crystals. By* PROFESSOR MILLER.

INTRODUCTION.

1. The law to which the mutual inclinations of the faces and cleavage planes of a crystal are subject, as enunciated by a large majority of the writers on Crystallography, is essentially embodied in the following statement :—

If through any point within a crystal planes be drawn parallel to each of its faces and cleavage planes, and any three of the straight lines in which these planes intersect one another, not being in one plane, be taken for axes, the equation to any face or cleavage plane of the crystal will be

$$h\,\frac{x}{a} + k\,\frac{y}{b} + l\,\frac{z}{c} = d,$$

where a, b, c are any three straight lines the ratios of which depend upon the species of the crystal, and the selection of axes, d is any positive quantity, and h, k, l are any positive or negative integers one or two of which may be zero.

A very different method was invented by Grassmann, who tells us that the difficulty of following the combinations of planes in the imagination, led him to the idea of substituting for the plane surfaces of crystals, normals to those surfaces or rays as he terms them. In other words, instead of the crystal he employs its reciprocal figure, adopting the definition of reciprocal figures given by Professor James Clerk Maxwell in the Philosophical Magazine for April, 1864. Grassmann was followed in the use of this method by Hessell in the Article *Krystall* in Gehler's Physikalisches Wörterbuch, reprinted separately under the title Krystallometrie, Leipzig, 1831; by Frankenheim in 1832, in a very elegant investigation of certain geometrical theorems, Einige Sätze aus der Geometrie der geraden Linie, Crelle, B. 8, S. 178; and lastly by Uhde, Versuch einer genetischen Entwickelung der mechanischen Krystalliza-tions-Gesetze, Bremen, 1833. Later, however, this method appears to have been treated with a neglect it little deserves, for it possesses all the advantages of simplicity claimed for it by Grassmann, it leads directly to Neumann's representation of a crystal by the poles of its faces, and admits readily of the application of analytical geometry of three dimensions, ordinary geometry, or spherical trigonometry, in the investigation of the geometrical properties of crystalline forms. And though it may

ot have led to any result that has not been obtained by the
nore usual method of treating the subject, an acquaintance with
 can hardly fail to impart a clearer insight into the compli-
ated relations of crystalline forms, and afford a fresh instance of
he truth of the remark made by Sir John Herschel (Astronomy,
th Edition, p. 6) that it is always of advantage to present any
iven body of knowledge to the mind in as great a variety of
ghts as possible.

2. According to Grassmann, if from any point within a
rystal lines be drawn normal to the several faces of the crystal,
nd any three of these normals, not all in one plane, be taken
or axes, the equations to any other normal will be

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma},$$

here α, β, γ are three straight lines the ratios of which depend
pon the species of the crystal and the selection of axes, and
, k, l are any integers either positive or negative or zero, one at
east remaining finite. That these two enunciations lead to
dentical results, though not at first sight obvious, admits of an
asy proof.

3. In fig. 1 let O be any point within a crystal. Let the
urface of a sphere described round O as a centre meet the
xes in X, Y, Z. Let ABC be the polar triangle of XYZ, and
herefore OA, OB, OC radii normal to the faces 1 0 0, 0 1 0,
 0 1; P the pole of the face $h\,k\,l$; and a, b, c the parameters of
he crystal.

Let L be the intersection of the great circles BC, AP.
hrough any point R in the straight line OP draw RQ parallel
o OA, meeting the straight line OL in Q. Through Q draw
N parallel to OB meeting OC in N. Let $QR = x$, $NQ = y$,
$N = z$. It is proved in my Tract on Crystallography (4) that

$$\frac{k}{b}\sin BAP = \frac{l}{c}\sin CAP, \frac{l}{c}\sin CBP = \frac{h}{a}\sin ABP,$$

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$$\frac{h}{a} \sin A\, CP = \frac{k}{b} \sin BCP.$$

But $\qquad \sin AB \sin BAP = \sin BL \sin BLP,$

and $\qquad \sin CA \sin CAP = \sin CL \sin CLP.$

Hence

$$\frac{y}{z} = \frac{\sin NOQ}{\sin OQN} = \frac{\sin CL}{\sin BL} = \frac{k}{b}\frac{c}{l}\frac{\sin CA}{\sin AB}.$$

In like manner $\qquad \dfrac{z}{x} = \dfrac{l}{c}\dfrac{a}{h}\dfrac{\sin AB}{\sin BC}.$

Also

$$\frac{\sin BC}{\sin A} = \frac{\sin CA}{\sin B} = \frac{\sin AB}{\sin C}, \quad \frac{\sin YZ}{\sin X} = \frac{\sin ZX}{\sin Y} = \frac{\sin XY}{\sin Z};$$

and, since ABC is the polar triangle of XYZ, $\sin YZ = \sin A$, $\sin ZX = \sin B$, $\sin XY = \sin C$. Therefore the equations to OR, a normal to the face $h\,k\,l$, will be

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma},$$

where α, β, γ are three straight lines subject to one of the four indentical conditions

$$\frac{a\alpha}{\sin BC} = \frac{b\beta}{\sin CA} = \frac{c\gamma}{\sin AB}, \qquad \frac{a\alpha}{\sin A} = \frac{b\beta}{\sin B} = \frac{c\gamma}{\sin C},$$

$$\frac{a\alpha}{\sin YZ} = \frac{b\beta}{\sin ZX} = \frac{c\gamma}{\sin XY}, \qquad \frac{a\alpha}{\sin X} = \frac{b\beta}{\sin Y} = \frac{c\gamma}{\sin Z}.$$

It is evident that OR is the diagonal of a parallelopiped having its edges in the lines OA, OB, OC, and respectively proportional to $h\alpha$, $k\beta$, $l\gamma$.

Let G be the pole 1 1 1.

Then

$$\frac{1}{b} \sin BAG = \frac{1}{c} \sin CAG, \quad \frac{1}{c} \sin CBG = \frac{1}{a} \sin ABG,$$

$$\frac{1}{a} \sin ACG = \frac{1}{b} \sin BCG.$$

Therefore

$$k\frac{\sin BAP}{\sin BAG} = l\frac{\sin CAP}{\sin CAG}, \quad l\frac{\sin CBP}{\sin CBG} = h\frac{\sin ABP}{\sin ABG},$$

$$h \frac{\sin ACP}{\sin ACG} = k \frac{\sin BCP}{\sin BCG}.$$

This is equivalent to the form in which the law was enunciated by Gauss (C. F. Gauss, Werke, Band II. S. 308).

4. It remains to be seen whether the symbol of a zone has any geometrical signification when the normals to the faces are referred to OA, OB, OC as axes.

It appears from what precedes, interchanging a, b, c and α, β, γ, that

$$\frac{x}{ha} = \frac{y}{kb} = \frac{z}{lc}, \text{ and } h\frac{x}{\alpha} + k\frac{y}{\beta} + l\frac{z}{\gamma} = 0,$$

being the equations to a line and plane, one referred to the axes OX, OY, OZ, and the other to the axes OA, OB, OC, the line will be normal to the plane. But (Tract 200), the equations to the zone-axis u v w referred to the axes OX, OY, OZ, are

$$\frac{x}{ua} = \frac{y}{vb} = \frac{z}{wc}.$$

Hence, a plane through O, normal to the axis of the zone u v w, when referred to the axes OA, OB, OC, will have for its equation

$$u\frac{x}{\alpha} + v\frac{y}{\beta} + w\frac{z}{\gamma} = 0.$$

Let a plane parallel to the zone plane u v w meet OA, OB, OC in U, V, W. Then it is evident that

$$u\frac{OU}{\alpha} = v\frac{OV}{\beta} = w\frac{OW}{\gamma}.$$

Let the axis of the zone u v w meet the surface of the sphere in K. It is easily seen that

$$\frac{\alpha}{u}\cos AK = \frac{\beta}{v}\cos BK = \frac{\gamma}{w}\cos CK.$$

5. It appears then that the notation for faces and zones suggested by the equations to the faces and zone-axes, when the crystal is referred to three zone-axes as axes of coordinates, is equally applicable when the crystal is represented by rays normal to its faces, and these are referred to three such rays as

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axes. Since the notation is the same in either case, it follows that the expressions for the geometric relations between faces and zones, in terms of their respective indices, will be absolutely the same whether we refer the faces to three zone-axes, or their rays to the three corresponding rays as axes of coordinates.

6. Instead of deducing the properties of a system of rays referred to three rays as axes, from those of faces referred to three zone-axes, it will be better to investigate the properties of Grassmann's system of rays without any reference to the crystal it is intended to represent. Having once established the properties of a purely geometric system of rays we may proceed to the consideration of crystals, avoiding the mistake sometimes made of anticipating the result of a geometrical investigation in the enunciation of a physical law, and assert that measurements of the angles between normals to the faces of crystals show that these normals are subject to the law according to which the system of rays was constructed, and that, consequently, all the geometrical properties of such a system of rays are properties of the rays drawn from any point within the crystal normal to its faces.

The analytical investigation is followed, first, by one in which ordinary geometry is used; and, afterwards, by an investigation of the properties of a system of points on the surface of a sphere, the points being the intersections of the rays with the surface of a sphere described round the origin of the system as a centre. Those propositions are omitted the investigation of which is the same whether we employ the polyhedral solid or its reciprocal figure.

ANALYTICAL INVESTIGATION OF THE PROPERTIES OF A SYSTEM OF RAYS.

Rays.

7. Let OA, OB, OC be any three straight lines not all in one plane; α, β, γ any three straight lines in a given proportion;

h, k, l any three integers positive or negative or zero, one of them at least remaining finite; let OA, OB, OC be taken as co-ordinate axes; and let a system of lines be constructed by giving different values to h, k, l in the equations

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma}.$$

That portion of any line defined by the preceding equations, which lies on one side of the origin, will be called a ray. The ray containing the point $x = h\alpha$, $y = k\beta$, $z = l\gamma$ will be denoted by the symbol $h\ k\ l$. The ray containing the point $x = -h\alpha$, $y = -k\beta$, $z = -l\gamma$ will be denoted by the symbol $\bar{h}\ \bar{k}\ \bar{l}$. The integers $h\ k\ l$ will be called the indices of the ray; and the lines α, β, γ will be called the parameters of the system of rays thus constructed.

The lines OA, OB, OC are manifestly the rays $1\ 0\ 0$, $0\ 1\ 0$, $0\ 0\ 1$.

Zone-planes.

8. The equations to the rays $h\ k\ l$, $p\ q\ r$ are

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma}, \text{ and } \frac{x}{p\alpha} = \frac{y}{q\beta} = \frac{z}{r\gamma}.$$

Hence the equation to the plane containing the rays $h\ k\ l$, $p\ q\ r$ will be

$$\mathrm{u}\frac{x}{\alpha} + \mathrm{v}\frac{y}{\beta} + \mathrm{w}\frac{z}{\gamma} = 0,$$

where $\quad \mathrm{u} = kr - lq, \ \mathrm{v} = lp - hr, \ \mathrm{w} = hq - kp.$

This plane will be called a zone-plane, and will be denoted by the symbol u v w. The quantities u, v, w will be called its indices. They are evidently positive or negative integers one or two of which may be zero.

The symbols of the zone-planes BOC, COA, AOB are $1\ 0\ 0$, $0\ 1\ 0$, $0\ 0\ 1$ respectively.

The intersection of any two zone-planes is a ray.

9.   The zone-planes h k l, p q r have for their equations

$$\text{h}\frac{x}{\alpha} + \text{k}\frac{y}{\beta} + \text{l}\frac{z}{\gamma} = 0, \text{ and } \text{p}\frac{x}{\alpha} + \text{q}\frac{y}{\beta} + \text{r}\frac{z}{\gamma} = 0.$$

The equations to the intersections of these planes will be

$$\frac{x}{u\alpha} = \frac{y}{v\beta} = \frac{z}{w\gamma},$$

where $u = \text{kr} - \text{lq}, \ v = \text{lp} - \text{hr}, \ w = \text{hq} - \text{kp}.$

The quantities u, v, w are obviously integers, and therefore the intersection of any two zone-planes is a ray.

Condition that a ray may lie in a zone-plane.

10.   Let the zone-plane p q r contain the ray $u\, v\, w$.   The equations to the zone-plane and ray are

$$\text{p}\frac{x}{\alpha} + \text{q}\frac{y}{\beta} + \text{r}\frac{z}{\gamma} = 0, \text{ and } \frac{x}{u\alpha} = \frac{y}{v\beta} = \frac{z}{w\gamma}.$$

Therefore, since the plane contains the ray,

$$\text{p}u + \text{q}v + \text{r}w = 0.$$

Portions of two rays cut off by parallels to two zone-planes.

11.   In fig. 2 let OQ, OS be the rays $h\, k\, l$, $u\, v\, w$, and let the zone-plane QOS intersect the zone-planes e f g, p q r, in OP, OR.   Let the planes having for their equations

$$\text{e}\frac{x}{\alpha} + \text{f}\frac{y}{\beta} + \text{g}\frac{z}{\gamma} = m, \ \ \text{p}\frac{x}{\alpha} + \text{q}\frac{y}{\beta} + \text{r}\frac{z}{\gamma} = n,$$

and therefore parallel to the zone-planes e f g, p q r, meet the ray $h\, k\, l$ in D, Q, and the ray $u\, v\, w$ in F, S.   Let planes passing through D, Q, F, S, parallel to the zone-planes 1 0 0, meet the ray 1 0 0 in d, q, f, s.   Then Od, Oq, Of, Os will be the values of x at the points D, Q, F, S.   Therefore, since the equations to the rays $h\, k\, l$, $u\, v\, w$ are

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma}, \text{ and } \frac{x}{u\alpha} = \frac{y}{v\beta} = \frac{z}{w\gamma},$$

ve shall have

$$(eh + fk + gl)\, Od = mh\mathbf{1}, \quad (ph + qk + rl)\, Oq = nh\mathbf{1},$$

$$(eu + fv + gw)\, Of = mu\mathbf{1}, \quad (pu + qv + rw)\, Os = nu\mathbf{1}.$$

But $Od : Oq = OD : OQ$, and $Of : Os = OF : OS$. Therefore

$$\frac{eu + fv + gw}{eh + fk + gl}\, \frac{OF}{OD} = \frac{pu + qv + rw}{ph + qk + rl}\, \frac{OS}{OQ}.$$

When only one of the rays OP, OR lies between OQ and OS, three of the points d, q, f, s will be on one side of O, and the fourth on the other side. Therefore $Of . Os : Oq . Od$ will be negative. When OP, OR are both without the angle OOS, or both within it, the points d, q, f, s will either be all on one side of O, or two on one side and two on the other side, and $Of . Os : Oq . Od$ will be positive. Hence the expression

$$\frac{eh + fk + gl}{eu + fv + gw}\, \frac{pu + qv + rw}{ph + qk + rl}$$

will be positive except when one only of the rays OP, OR lies between OQ and OS.

Anharmonic ratio of four rays in one zone-plane.

12.   Since DF, QS are parallel to OP, OR respectively,

$$\sin POQ : \sin POS = \sin D : \sin F = OF : OD,$$

and $\sin ROQ : \sin ROS = \sin Q : \sin S = OS : OQ.$

Hence $\dfrac{\sin POQ}{\sin POS}\, \dfrac{\sin ROS}{\sin ROQ} = \dfrac{eh + fk + gl}{eu + fv + gw}\, \dfrac{pu + qv + rw}{ph + qk + rl}$,

where OP, OQ, OR, OS are four rays in one zone-plane; e f g, p q r the symbols of zone-planes containing the rays OP, OR; and h k l, u v w the symbols of the rays OQ, OS.

Anharmonic ratio of four zone-planes intersecting one another in one ray.

13.   Retaining the notation of (12), let the zone-planes e f g, p q r intersect in the ray OK; and let a plane through O, in fig. 3,

normal to OK, meet the zone-planes e f g, p q r in Op, Or; KOQ, KOS in Oq, Os; and planes through DF, QS parallel to OK, in df, qs. Then df, qs will be parallel to Op, Or;

$$Od : Oq = OD : OQ\; ; \text{ and } \; Of : Os = OF : OS.$$

Therefore $\quad \sin pOq : \sin pOs = \sin d : \sin f = Of : Od,$

and $\quad\quad \sin rOq : \sin rOs = \sin q : \sin s = Os : Oq.$

Hence $\dfrac{\sin pOq}{\sin pOs}\dfrac{\sin rOs}{\sin rOq} = \dfrac{eh + fk + gl}{eu + fv + gw}\dfrac{pu + qv + rw}{ph + qk + rl}$,

where KOP, KOQ, KOR, KOS are four zone-planes intersecting one another in one ray; e f g, p q r the symbols of KOP, KOR; $h\,k\,l, u\,v\,w$ the symbols of rays contained in the zone-planes KOQ, KOS; pOq, pOs the angles which KOP makes with KOQ, KOS; and rOq, rOs the angles which KOR makes with KOQ, KOS.

Since the order of the zone-planes KOP, KOQ, KOR, KOS is the same as that of the rays OP, OQ, OR, OS, it follows from (11) that the expression which forms the right-hand side of the preceding equation is positive except when one only of the zone-planes KOP, KOR lies between the other two.

Indices of a ray when the axes are changed.

14. Let planes parallel to the zone-planes e f g, h k l, p q r meet the ray $m\,n\,o$ in D, L, Q, and the ray $u\,v\,w$ in F, N, S. Then (11)

$$\dfrac{eu + fv + gw}{em + fn + go}\dfrac{OF}{OD} = \dfrac{hu + kv + lw}{hm + kn + lo}\dfrac{ON}{OL} = \dfrac{pu + qv + rw}{pm + qn + ro}\dfrac{OS}{OQ}.$$

Let the zone-planes h k l, p q r intersect in the ray OA'; the zone-planes p q r, e f g in the ray OB'; and the zone-planes e f g, h k l in the ray OC'. And let $m'\,n'\,o', u'\,v'\,w'$ be the symbols of the rays OQ, OS when referred to the rays OA', OB', OC' as axes. The symbols of the zone-planes e f g, h k l,

p q r when referred to the new axes will be 1 0, 0, 0 1 0, 0 0 1 respectively. Therefore (11)

$$\frac{u'}{m'}\frac{OF}{OD} = \frac{v'}{n'}\frac{ON}{OL} = \frac{w'}{o'}\frac{OS}{OQ}.$$

Hence, comparing corresponding terms, two equations are obtained which are satisfied by making

$$m' = em + fn + go, \quad u' = eu + fv + gw,$$
$$n' = hm + kn + lo, \quad v' = hu + kv + lw,$$
$$o' = pm + qn + ro, \quad w' = pu + qv + rw.$$

The coefficients of u, v, w are integers, therefore u', v', w', the indices of the ray OS when referred to the rays OA', OB', OC' as axes, are also integers. Hence, the rays of the system are subject to the same law when referred to any three rays as axes, as when referred to the original axes.

Indices of a zone-plane when the axes are changed.

15. Let the rays $e f g$, $h k l$, $p q r$ meet a plane parallel to the zone-plane m n o in D, E, F, and a plane parallel to the zone-plane u v w in R, S, T. Then (11)

$$\frac{ue + vf + wg}{me + nf + og}\frac{OR}{OD} = \frac{uh + vk + wl}{mh + nk + ol}\frac{OS}{OE} = \frac{up + vq + wr}{mh + nq + or}\frac{OT}{OF}.$$

Let m'n'o', u'v'w' be the symbols of the zone-planes parallel to *DEF, RST*, when referred to the rays $e f g$, $h k l$, $p q r$ as axes. The new symbols of the rays OR, OS, OT will be 1 0 0, 0 1 0, 0 0 1. Therefore (11)

$$\frac{u'}{m'}\frac{OR}{OD} = \frac{v'}{n'}\frac{OS}{OE} = \frac{w'}{o'}\frac{OT}{OF}.$$

Hence, comparing corresponding terms, two equations are obtained which are satisfied by making

$$m' = em + fn + go, \quad u' = eu + fv + gw,$$
$$n' = hm + kn + lo, \quad v' = hu + kv + lw,$$
$$o' = pm + qn + ro, \quad w' = pu + qv + rw.$$

Geometrical Investigation of the Properties of a System of Rays.

Rays.

16. In fig. 4 let O be the origin of a system of rays; OA, OB, OC the rays $1\,0\,0$, $0\,1\,0$, $0\,0\,1$; OR the ray $h\,k\,l$; HKL a parallelopiped having its edges in OA, OB, OC, and having OR for a diagonal. Then (7), since OH, OK, OL are the values of x, y, z for the point R, we shall have

$$\frac{OH}{h\alpha} = \frac{OK}{k\beta} = \frac{OL}{l\gamma}.$$

Zone-planes.

17. In OA, fig. 5, take $OU = -\alpha$, and therefore measured from O in the direction opposite to A. Through U draw UM, US parallel to the rays $h\,k\,l$, $p\,q\,r$ respectively, meeting the plane BOC in M, S. Let MS meet OB in V, and OC in W. Draw MD, SG parallel to OC meeting OV in D, G. The lines UM, US are parallel to the rays $h\,k\,l$, $p\,q\,r$, therefore, observing that since $OU = -\alpha$, $UO = \alpha$,

$$\frac{UO}{h\alpha} = \frac{OD}{k\beta} = \frac{DM}{l\gamma}, \text{ and } \frac{UO}{p\alpha} = \frac{OG}{q\beta} = \frac{GS}{r\gamma}.$$

Hence

$$OD = \frac{k}{h}\beta, \; DM = \frac{l}{h}\gamma, \;\; OG = \frac{q}{p}\beta, \;\; GS = \frac{r}{p}\gamma.$$

The lines DM, GS are parallel to OW, therefore

$$OW : OV = DM : DV = DM - GS : OG - OD;$$

consequently

$$(hq - kp)\beta.OW = (lp - hr)\gamma.OV, \; h(lp - hr).DV = l(hq - kp)\beta,$$
$$(lp - hr).OV = (lq - kr)\beta, \; (hq - kp).OW = (lq - kr)\gamma.$$

Hence, if a plane parallel to the rays $h\,k\,l$, $p\,q\,r$ meet OA, OB, OC in UVW,

$$\frac{u}{\alpha}\,OU = \frac{v}{\beta}\,OV = \frac{w}{\gamma}\,OW,$$

where $\quad u = kr - lq, \quad v = lp - hr, \quad w = hq - kp.$

A plane through the rays $h\,k\,l$, $p\,q\,r$, and therefore parallel to the plane UVW, will be called a zone-plane, and will be denoted by the symbol u v w, or by any three integers respectively proportional to u, v, w; and the integers u, v, w, or any three proportional integers, will be called the indices of the zone-plane.

The intersection of any two zone-planes is a ray.

18. In OB, fig. 6, take $OB = \beta$. Let planes through B parallel to the zone-planes h k l, p q r intersect one another in the line BM which meets the plane COA in M; and let them meet OC in L, R, and OA in H, P. Then

$$\frac{h}{\alpha}\,OH = \frac{k}{\beta}\,OB = \frac{l}{\gamma}\,OL, \text{ and } \frac{p}{\alpha}\,OP = \frac{q}{\beta}\,OB = \frac{r}{\gamma}\,OR.$$

Therefore

$\quad$ l . $OL = $ kγ, h . $OH = $ kα, r . $OR = $ qγ, p . $OP = $ qα.

Hence

$\quad\quad$ lr . $LR = $ (kr $-$ lq)γ, $\quad$ hp . $HP = $ (hq $-$ kp) α.

But (Tract 187)

$$HM \,.\, OP \,.\, LR = HP \,.\, OR \,.\, LM.$$

Therefore, putting

$$u = \text{kr} - \text{lq}, \quad v = \text{lp} - \text{hr}, \quad w = \text{hq} - \text{kp},$$

we have

$wl \,.\, LM = u$h $.\, HM$, $\,wl \,.\, LH = - v$k $.\, HM$, uh $.\, LH = - v$k $.\, LM$.

Draw MD parallel to OC meeting OA in D. By similar triangles $OD : LM = OH : LH$, and $DM : HM = OL : LH$. Hence $-v \,.\, OD = u\alpha$, and $-v \,.\, DM = w\gamma$. Draw MF equal and parallel to OB on the opposite side of the plane LOH. Then $-v \,.\, MF = v \,.\, OB = v\beta$. The line OF being parallel to BM, is

the intersection of the zone-planes h k l, p q r, and is evidently the diagonal of a parallelopiped the edges of which are respectively coincident with the axes of the system of rays, and equal to OD, MF, DM, and therefore proportional to

$$-v \cdot OD, \ -v \cdot MF, \ -v \cdot DM, \ \text{or to } u\alpha, \ v\beta, \ w\gamma.$$

Since u, v, w are integers, the line OF, in which the zone-planes h k l, p q r intersect, is a ray of the system, having $u \, v \, w$ for its symbol, where

$$u = \text{kr} - \text{lq}, \quad v = \text{lp} - \text{hr}, \quad w = \text{hq} - \text{kp}.$$

Portions of two rays cut off by parallels to two zone-planes.

19. Let a plane parallel to the zone-plane p q r meet the axes in I, J, K, fig. 7, and the ray $u \, v \, w$ in S. Draw KS meeting IJ in N, IS meeting JK in L, and ST parallel to OI, meeting the plane JOK in T. The symbols of the rays OK, OI, OS are 0 0 1, 1 0 0, $u \, v \, w$ respectively. Therefore the symbol of the zone-plane KOS will be $v \, u \, 0$, and that of the zone-plane IOS will be $0 \, \bar{w} \, v$. The plane IJK is parallel to the zone-plane p q r. Hence the line KN will be parallel to the ray

$$-ru, \quad -rv, \quad pu + qv,$$

and the line IL will be parallel to the ray

$$qv + rw, \quad -pv, \quad -pw.$$

The lines KN, IL are in the plane IJK, therefore (18)

$$pu \cdot IN = qv \cdot JN, \text{ and } qv \cdot JK = (qv + rw) \cdot KL.$$

But (Tract 187) $\quad IS \cdot KL \cdot JN = SL \cdot JK \cdot IN.$

Hence $\quad pu \cdot IS = (qv + rw) \cdot SL,$

and therefore $\quad pu \cdot IL = (pu + qv + rw) \, SL.$

But $\quad ST : OI = SL : IL.$

Therefore $\quad pu \cdot OI = (pu + qv + rw) \cdot ST.$

In like manner, if a plane parallel to the zone-plane e f g meet

OI in E, and OS in F, and FG be drawn parallel to OI meeting the plane JOK in G, we shall have

$$\text{e}u \,.\, OE = (\text{e}u + \text{f}v + \text{g}w) \,.\, FG.$$

But $$OS \,:\, OF = ST \,:\, FG.$$

Therefore

$$(\text{e}u + \text{f}v + \text{g}w) \,.\, OF \,:\, (\text{p}u + \text{q}v + \text{r}w) \,.\, OS = \text{e} \,.\, OE \,:\, \text{p} \,.\, OI.$$

Hence if the ray $h\,k\,l$ meet planes parallel to the zone-planes e f g, p q r in the points D, Q, we shall have

$$(\text{e}h + \text{f}k + \text{g}l) \,.\, OD \,:\, (\text{p}h + \text{q}k + \text{r}l) \,.\, OQ = \text{e} \,.\, OE \,:\, \text{p} \,.\, OI.$$

Therefore

$$\frac{\text{e}u + \text{f}v + \text{g}w}{\text{e}h + \text{f}k + \text{g}l} \; \frac{OF}{OD} = \frac{\text{p}u + \text{q}v + \text{r}w}{\text{p}h + \text{q}k + \text{r}l} \; \frac{OS}{OQ}.$$

From this equation the anharmonic ratio of four rays in one zone-plane, of four zone-planes intersecting one another in one ray, and the indices of rays and zone-planes when the axes are changed, may be found as in (12), (13), (14), (15).

Properties of a System of Points on the Surface of a Sphere.

Poles.

20. Let the surface of a sphere having its centre in the origin of the system of rays meet the rays 1 0 0, 0 1 0, 0 0 1, 1 1 1 in A, B, C, G, and the ray $h\,k\,l$ in P, fig. 1. Let the great circle AP meet the great circle BC in L. From any point R in OP draw RQ parallel to OA meeting OL in Q. Draw QN parallel to OB meeting OC in N. Then (7), since QR, NQ, ON are the values of x, y, z at R,

$$\frac{QR}{h\alpha} = \frac{NQ}{k\beta} = \frac{ON}{l\gamma}.$$

But

$$ON \,:\, NQ = \sin BL \,:\, \sin CL = \sin AB \sin BAP \,:\, \sin CA \sin CAP.$$

Therefore

$$k\beta \sin AB \sin BAP = l\gamma \sin CA \sin CAP.$$

In like manner

$$l\gamma \sin BC \sin CBP = h\alpha \sin AB \sin ABP,$$

and $\qquad h\alpha \sin CA \sin ACP = k\beta \sin BC \sin BCP.$

Hence, $\qquad \beta \sin AB \sin BAG = \gamma \sin CA \sin CAG,$

$$\gamma \sin BC \sin CBG = \alpha \sin AB \sin ABG,$$

$$\alpha \sin CA \sin ACG = \beta \sin BC \sin BCG.$$

Therefore

$$k\frac{\sin BAP}{\sin BAG} = l\frac{\sin CAP}{\sin CAG},$$

$$l\frac{\sin CBP}{\sin CBG} = h\frac{\sin ABP}{\sin ABG},$$

$$h\frac{\sin ACP}{\sin ACG} = k\frac{\sin BCP}{\sin BCG}.$$

The point P will be called a pole, and will be denoted by the symbol of the ray intersected in that point by the surface of the sphere. The points A, B, C, G are the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1 respectively.

Zone-circles.

21. In fig. 8 let A, B, C, G, P be the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1, uvw respectively. Let a great circle passing through P meet the great circles BC, CA, AB in D, E, F respectively. Then (Townsend's *Modern Geometry*, 82, Cor. 3), having regard to the signs of the six arcs,

$$\sin DP \sin FE + \sin PF \cdot \sin DE + \sin FD \cdot \sin PE = 0.$$

Therefore

$$\frac{\sin FE}{\sin FD}\frac{\sin DP}{\sin PE} + \frac{\sin DE}{\sin FD}\frac{\sin PF}{\sin PE} + 1 = 0,$$

whence

$$\frac{\sin ACF}{\sin FCB}\frac{\sin BCP}{\sin ACP} + \frac{\sin DAC}{\sin BAD}\frac{\sin BAP}{\sin CAP} + 1 = 0.$$

But (20)

$$u \frac{\sin ACP}{\sin ACG} = v \frac{\sin BCP}{\sin BCG}, \text{ and } v \frac{\sin BAP}{\sin BAG} = w \frac{\sin CAP}{\sin CAG}.$$

Therefore, putting

$$\frac{u}{v} = \frac{\sin ACF}{\sin FCB} \frac{\sin BCG}{\sin ACG}, \text{ and } \frac{w}{v} = \frac{\sin DAC}{\sin BAD} \frac{\sin BAG}{\sin CAG},$$

$$uu + vv + ww = 0.$$

Let the great circle EF pass through the poles $h\,k\,l$, $p\,q\,r$, not being opposite extremities of a diameter of the sphere.

Then $\quad uh + vk + wl = 0$, and $up + vq + wr = 0$.

These two equations are satisfied by making

$$u = kr - lq, \qquad v = lp - hr, \qquad w = hq - kp.$$

The great circle passing through the poles $h\,k\,l$, $p\,q\,r$ will be called a zone-circle, and will be denoted by the symbol u v w, or by any three integers in the same proportion.

Condition that a pole may be in a zone-circle.

22. It appears from (21) that when the zone-circle u v w passes through the pole $u\,v\,w$, we have

$$uu + vv + ww = 0.$$

Any integral values of u, v, w that satisfy this equation are the indices of a pole in the zone-circle u v w, and any integral values of u, v, w that satisfy it are the indices of a zone-circle passing through the pole $u\,v\,w$.

The intersections of any two zone-circles are poles.

23. Let the zone-circles h k l, p q r intersect in the points P, P'. If it be possible, let P be the pole $u\,v\,w$. Then (22)

$$hu + kv + lw = 0, \text{ and } pu + qv + rw = 0.$$

These equations are satisfied by making

$$u = \mathrm{kr} - \mathrm{lq}, \quad v = \mathrm{lp} - \mathrm{hr}, \quad w = \mathrm{hq} - \mathrm{kp}.$$

It is evident that u, v, w are integers, therefore P is the pole $u\,v\,w$, and P' is the $\bar{u}\,\bar{v}\,\bar{w}$.

Relation between the arcs AK, BK, CK, K being a pole of the zone-circle EF.

24. Let u v w be the symbol of the zone-circle EF, and let K be the pole of the zone-circle EF nearest to C. Then

$$\cos AK = -\sin AE \sin E, \quad \cos BK = -\sin BD \sin D,$$
$$\cos CK = \sin CD \sin D = \sin CE \sin E.$$

The symbol of D is $0\,\mathrm{w}\,\bar{\mathrm{v}}$, and the symbol of E is $\mathrm{w}\,0\,\bar{\mathrm{u}}$. Therefore (20)

$$\frac{\cos AK}{\cos CK} = -\frac{\sin AE}{\sin CE} = -\frac{\sin AB}{\sin BC}\frac{\sin ABE}{\sin CBE} = \frac{\mathrm{u}\gamma}{\mathrm{w}\alpha},$$

and

$$\frac{\cos BK}{\cos CK} = -\frac{\sin BD}{\sin CD} = -\frac{\sin AB}{\sin CA}\frac{\sin BAD}{\sin CAD} = \frac{\mathrm{v}\gamma}{\mathrm{w}\beta};$$

Whence

$$\frac{\alpha}{\mathrm{u}}\cos AK = \frac{\beta}{\mathrm{v}}\cos BK = \frac{\gamma}{\mathrm{w}}\cos CK.$$

Anharmonic ratio of four zone-circles passing through one pole.

25. In fig. 9 let A, B, C be the poles $1\,0\,0$, $0\,1\,0$, $0\,0\,1$ respectively; KP the zone-circle e f g intersecting the zone-circles CA in M; KR the zone-circle p q r intersecting the zone-circle BC in N; Q the pole $h\,k\,l$; S the pole $u\,v\,w$. Let the zone-circles KQ, KS intersect the zone-circle MN in T, V; also let $\zeta\,\eta\,\theta$, $\phi\,\chi\,\psi$ be the symbols of T, V respectively. Then (21), (23) the symbol of M will be $\mathrm{g}\,0\,\bar{\mathrm{e}}$, the symbol of N will be $0\,\mathrm{r}\bar{\mathrm{q}}$, the symbol of MN will be er gq gr, the symbol of K will be $\mathrm{fr} - \mathrm{gq}\ \mathrm{gp} - \mathrm{er}\ \mathrm{eq} - \mathrm{fp}$, and the symbol of KQ will be

$$k\,\mathrm{eq} - k\,\mathrm{fp} - l\,\mathrm{gp} + l\,\mathrm{er},$$
$$l\,\mathrm{fr} - l\,\mathrm{gq} - h\,\mathrm{eq} + h\,\mathrm{fp},$$
$$h\,\mathrm{gp} - h\,\mathrm{er} - k\,\mathrm{fr} + k\,\mathrm{gq}.$$

Hence (23)

$$\zeta = \mathrm{g}\,(\mathrm{gq} - \mathrm{fr})\,(\mathrm{p}h + \mathrm{q}k + \mathrm{r}l),$$
$$\eta = \mathrm{r}\,(\mathrm{er} - \mathrm{gp})\,(\mathrm{e}h + \mathrm{f}k + \mathrm{g}l).$$

In like manner

$$\phi = \mathrm{g}\,(\mathrm{gq} - \mathrm{fr})\,(\mathrm{p}u + \mathrm{q}v + \mathrm{r}w),$$
$$\chi = \mathrm{r}\,(\mathrm{er} - \mathrm{gp})\,(\mathrm{e}u + \mathrm{f}v + \mathrm{g}w).$$

But
$$\frac{\sin PKQ}{\sin RKQ}\,\frac{\sin RKS}{\sin PKS} = \frac{\sin MT}{\sin NT}\,\frac{\sin NV}{\sin MV}$$
$$= \frac{\sin ACT}{\sin BCT}\,\frac{\sin BCV}{\sin ACV} = \frac{\eta}{\zeta}\,\frac{\phi}{\chi}.$$

Therefore

$$\frac{\sin PKQ}{\sin PKS}\,\frac{\sin RKS}{\sin RKQ} = \frac{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}\,\frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l}.$$

Anharmonic ratio of four poles in one zone-circle.

26. Let the zone-circle QS meet the zone-circle KP in P, and the zone-circle KR in R. Then, since the anharmonic ratio of the points P, Q, R, S is the same as that of the arcs KP, KQ, KR, KS (Tract 16),

$$\frac{\sin PQ}{\sin PS}\,\frac{\sin RS}{\sin RQ} = \frac{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}\,\frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l}.$$

Sign of the expression forming the right-hand side of the final equations in (25) and (26).

27. In (25) the left-hand side of the final equation may be replaced by its equivalent

$$\frac{\cot PKS - \cot PKR}{\cot PKQ - \cot PKR},$$

and in (26) it may be replaced by

$$\frac{\cot PS - \cot PR}{\cot PQ - \cot PR}.$$

From the form of these expressions it is manifest that they are positive, and therefore also the expression forming the right-hand side of the equations in both cases, except when one only of the zone-circles KP, KR lies between Q and S.

8

POSITION OF ANY POLE IN EACH OF THE SIX SYSTEMS OF CRYSTALLIZATION.

Position of any pole in the cubic system.

28. In this system the arcs joining every two of the poles 1 0 0, 0 1 0, 0 0 1 are quadrants, and the arcs joining the pole 1 1 1 and each of the poles 1 0 0, 0 0 1, 0 0 1 are all equal. Let A, B, C, O, P be the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1, $h\,k\,l$ respectively. Then BC, CA, AB are quadrants, $OA = OB = OC$, and the right angles A, B, C are bisected by OA, OB, OC. Hence the equations in (20) become

$$\tan BAP = \frac{l}{k}, \quad \tan CBP = \frac{h}{l}, \quad \tan ACP = \frac{k}{h}.$$

But

$$\tan AP = \tan ACP \sec BAP,$$
$$\tan BP = \tan BCP \sec ABP,$$
$$\tan CP = \tan CBP \sec ACP.$$

Whence

$$(\cos AP)^2 = \frac{h^2}{h^2 + k^2 + l^2},$$
$$(\cos BP)^2 = \frac{k^2}{h^2 + k^2 + l^2},$$
$$(\cos CP)^2 = \frac{l^2}{h^2 + k^2 + l^2}.$$

Position of any pole in the pyramidal system.

29. In this system the arcs joining the poles 1 0 0, 0 1 0, 0 0 1 are quadrants, and the arcs joining the pole 1 1 1 and each of the poles 1 0 0, 0 1 0 are equal. Let A, B, C, G, P be the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1, $h\,k\,l$ respectively. Then BC, CA, AB are quadrants, and $AG = BG$. Consequently $BAG = ABG$, and $ACG = BCG$. The arc BG intersects CA in the pole 1 0 1. Putting the arc 0 0 1, 1 0 1 $= E$, and observing that the angles A, B, C are right angles, the equations in (20) become

$$\tan BAP = \frac{l}{k} \cot E, \quad \tan ABP = \frac{l}{h} \cot E, \quad \tan ACP = \frac{k}{h}.$$

$$\cot AP = \frac{h}{k} \cos BAP = \frac{h}{l} \tan E \cos CAP,$$

$$\cot BP = \frac{k}{l} \tan E \cos CBP = \frac{k}{h} \cos ABP,$$

$$\cot CP = \frac{l}{h} \cot E \cos ACP = \frac{l}{k} \cot E \cos BCP,$$

$$(\tan CP)^2 = \frac{h^2 + k^2}{l^2} (\tan E)^2.$$

The arc E may be taken for the element of the crystal.

Position of any pole in the rhombohedral system.

30.   In this system the arcs joining every two of the poles 1 0 0, 0 1 0, 0 0 1 are all equal, and the arcs joining the pole 1 1 1 and each of the poles 1 0 0, 0 1 0, 0 0 1 are all equal.

In fig. 10, let A, B, C, O be the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1 respectively; P the pole $h\,k\,l$. Let the zone-circles OA, OB, OC meet the zone-circles BC, CA, AB in D, E, F. Then, since $BC = CA = AB$ and $OA = OB = OC$, it is evident that BC, CA, AB are bisected in the points D, E, F; that $OD = OE = OF$; that the angles at D, E, F are right angles; that the symbols of D, E, F are 0 1 1, 1 0 1, 1 1 0 respectively; and that the six angles having their apices in O are each of 60°.

The symbol of OA is 0 1 $\bar{1}$, and that of OB is 1 0 $\bar{1}$. Therefore (27),

$$\frac{\cot AOP - \cot AOB}{\cot AOF - \cot AOB} = \frac{h - l}{k - l}.$$

But $AOB = 120°$, $AOF = 60°$. Therefore $\tan AOB = -\sqrt{3}$, $\tan AOF = \sqrt{3}$. Hence

$$\tan AOP = \frac{(k - l)\,\sqrt{3}}{2h - k - l}.$$

In like manner

$$\tan BOP = \frac{(l-h)\sqrt 3}{2k-l-h},$$

and

$$\tan COP = \frac{(h-k)\sqrt 3}{2l-h-k}.$$

Hence,

$$\cos AOP = \frac{2h-k-l}{\sqrt{\{2(k-l)^2 + 2(l-h)^2 + 2(h-k)^2\}}},$$

$$\cos BOP = \frac{2k-l-h}{\sqrt{\{2(k-l)^2 + 2(l-h)^2 + 2(h-k)^2\}}},$$

$$\cos COP = \frac{2l-h-k}{\sqrt{\{2(k-l)^2 + 2(l-h)^2 + 2(h-k)^2\}}}.$$

Let the zone-circle OP meet the zone-circle CA in H and the zone-circle AB in I. The symbol of I will be $h-l, k-l, 0$. The symbol of OB is $1\,0\,\bar 1$, and that of CA will be $0\,1\,0$. Therefore (27),

$$\frac{\cot OP - \cot OH}{\cot OI - \cot OH} = \frac{k}{k-l}.$$

Let $OA = D$. Then

$$\tan OE = \cos 60^\circ \tan OA = \tfrac12 \tan D;$$

$$\cot OH = \cot OE \cos EOP$$

$$= 2\cot D \frac{l+h-2k}{\sqrt{\{2(k-l)^2 + 2(l-h)^2 + 2(h-k)^2\}}},$$

$$\cot OI = \cot OF \cos FOP$$

$$= 2\cot D \frac{h+k-2l}{\sqrt{\{2(k-l)^2 + 2(l-h)^2 + 2(h-k)^2\}}}.$$

Hence

$$\tan OP = \frac{\sqrt{\{2(k-l)^2 + 2(l-h)^2 + 2(h-k)^2\}}}{2h+2k+2l}\tan D.$$

The arc D may be taken for the element of the crystal.

Position of any pole in the prismatic system.

31. In this system the arcs joining any two of the poles $1\,0\,0, 0\,1\,0, 0\,0\,1$ are quadrants. Let A, B, C, G, P be the poles $1\,0\,0, 0\,1\,0, 0\,0\,1, 1\,1\,1, h\,k\,l$ respectively. Then BC, CA, AB are quadrants.

The arcs AG, BG, CG meet the arcs BC, CA, AB in the poles $0\,1\,1$, $1\,0\,1$, $1\,1\,0$. Putting $0\,1\,0$, $0\,1\,1 = D$, $0\,0\,1$, $1\,0\,1 = E$, $1\,0\,0$, $1\,1\,0 = F$, and observing that the angles A, B, C are right angles, the equations in (20) become

$$\tan BAP = \frac{l}{k}\tan D,\ \tan CBP = \frac{h}{l}\tan E,\ \tan ACP = \frac{k}{h}\tan F,$$

$$\cot AP = \frac{h}{k}\cot F \cos BAP = \frac{h}{l}\tan E \cos CAP,$$

$$\cot BP = \frac{k}{l}\cot D \cos CBP = \frac{k}{h}\tan F \cos ABP,$$

$$\cot CP = \frac{l}{h}\cot E \cos ACP = \frac{l}{k}\tan D \cos BCP.$$

Any two of the arcs D, E, F may be taken for the elements of the crystal. They are connected by the equation

$$\tan D \,.\, \tan E \,.\, \tan F = 1.$$

Position of any pole in the oblique system.

32. In this system the arc joining the poles $1\,0\,0$, $0\,1\,0$, and the arc joining the poles $0\,1\,0$, $0\,0\,1$ are quadrants. Let A, B, C, G, P be the poles $1\,0\,0$, $0\,1\,0$, $0\,0\,1$, $1\,1\,1$, $h\,k\,l$ respectively. Let BG, BP meet CA in L, S. Then L will be the pole $1\,0\,1$, and S the pole $h\,0\,l$. The arcs AB, BC are quadrants, and consequently the angles ACB, CAB are right angles. Hence the equations in (20) become

$$\frac{\tan CAP}{\tan CAG} = \frac{k}{l},\quad \frac{\sin AL \sin CS}{\sin CL \sin AS} = \frac{h}{l},\quad \frac{\tan ACP}{\tan ACG} = \frac{k}{h}.$$

Putting

$$\tan\theta = \frac{h}{l}\frac{\sin CL}{\sin AL},$$

the arc AS will be given by the equation

$$\tan\left(AS - \tfrac{1}{2}AC\right) = \tan\tfrac{1}{2}AC \tan\left(\tfrac{1}{4}\pi - \theta\right).$$

But

$$\sin CL = \cot ACG \cot BG, \quad \sin AL = \cot CAG \cot BG,$$
$$\sin CS = \cot ACP \cot BP, \quad \sin AS = \cot CAP \cot BP.$$

Whence

$$\frac{\tan BP}{\tan BG} = \frac{h}{k}\frac{\sin CL}{\sin CS} = \frac{l}{k}\frac{\sin AL}{\sin AS}.$$

The arcs AL, BG, CL may be taken for the elements of the crystal.

Position of any pole in the anorthic system.

33. Let A, B, C, G, P be the poles $1\,0\,0$, $0\,1\,0$, $0\,0\,1$, $1\,1\,1$, $h\,k\,l$ respectively; and let AG, BG, CG meet BC, CA, AB in D, E, F, the poles $0\,1\,1$, $1\,0\,1$, $1\,1\,0$. It is easily seen that

$$\frac{\sin CA}{\sin AB}\frac{\sin CAG}{\sin BAG} = \frac{\sin CD}{\sin BD}, \quad \frac{\sin AB}{\sin BC}\frac{\sin ABG}{\sin CBG} = \frac{\sin AE}{\sin CE},$$
$$\frac{\sin BC}{\sin CA}\frac{\sin BCG}{\sin ACG} = \frac{\sin BF}{\sin AF}.$$

But (20),

$$k\frac{\sin BAP}{\sin BAG} = l\frac{\sin CAP}{\sin CAG}, \quad l\frac{\sin CBP}{\sin CBG} = h\frac{\sin ABP}{\sin ABG},$$
$$h\frac{\sin ACP}{\sin ACG} = k\frac{\sin BCP}{\sin BCG}.$$

Whence

$$\frac{\sin CAP}{\sin BAP} = \frac{k}{l}\frac{\sin AB}{\sin CA}\frac{\sin CD}{\sin BD},$$
$$\frac{\sin ABP}{\sin CBP} = \frac{l}{h}\frac{\sin BC}{\sin AB}\frac{\sin AE}{\sin CE},$$
$$\frac{\sin BCP}{\sin ACP} = \frac{h}{k}\frac{\sin CA}{\sin BC}\frac{\sin BF}{\sin AF}.$$

Putting tan θ, tan ϕ, tan ψ respectively for the right-hand sides of the preceding equations, we obtain

$$\tan (BAP - \tfrac{1}{2}BAC) = \tan \tfrac{1}{2}BAC \tan (\tfrac{1}{4}\pi - \theta),$$
$$\tan (CBP - \tfrac{1}{2}CBA) = \tan \tfrac{1}{2}CBA \tan (\tfrac{1}{4}\pi - \phi),$$
$$\tan (ACP - \tfrac{1}{2}ACB) = \tan \tfrac{1}{2}ACB \tan (\tfrac{1}{4}\pi - \psi).$$

Whence, knowing the segments BD, CD, CE, AE, AF, BF, the position of P can be found.

Any five of the segments of the sides of ABC may be taken for the elements of the crystal. For five of the segments being known, the remaining segment is given by the equation

$$\sin BD \sin CE \sin AF = \sin CD \sin AE \sin BF.$$

On the Association of Potton Sand Fossils with those of the Farringdon Gravels in a phosphatic deposit at Upware on the Cam; with an account of the Superposition of the Beds, and the significance of the Affinities of the Fossils. By Mr HARRY SEELEY, F.G.S.

ABSTRACT.

IN 1860 the author had traced the Galt by Swaffham fen, west of Wicken into Soham Mere; fossils were then collected and placed in the Woodwardian Museum. But though the beds over the Kimeridge clay and under the Galt are represented in Dr Fitton's section through Upware, they do not appear to have been seen again until the pits were opened for digging nodules of phosphate of lime last year. These have yielded about 120 species of fossils, chiefly mollusca and spunges, with a number of vertebrates. As a whole they recall beds in the same position in the north of Germany; in part they resemble with unexpected closeness the fauna of the Farringdon gravels; while

the resemblance to the fossils of Potton is such that nearly all Potton types of life have already occurred at the Wicken diggings. Potton however is rich in vertebrate remains and in the phosphatic casts of shells; at Wicken the silico-phosphatic concretions are smaller, and the mollusca, &c. for the most part preserve their carbonate of lime shells; moreover at Potton have occurred Cycadoidea microphylla, Cycad cones, cones of Pandanus and three species of Pinites, besides much wood mineralized, sometimes with phosphate of lime, sometimes with silex. At Wicken but little wood has occurred. All these facts appear to demonstrate that, assuming the phosphate beds at these places to be one and the same, then Potton was nearer to the old land, streams from which brought down the animals and plants, than was Wicken. This was assumed. Then it would follow that on these deposits being traced to the south-west they would become fluvio-marine and freshwater, and finally have no representative in that direction because of the interposition of dry land. Also if these remains were brought down by rivers (and they are in the same state of preservation as bones from the Wealden), the river banks on ceasing would become continuous with the sea shore of the land, along which would be distributed fragments of the rocks which formed the cliffs in those days, as well as rolled bones.

Such a pebble bed is found, and with it are mixed the nodules of phosphate of lime, the casts of shells, the sand nodules concreted with phosphate of lime, and most of the reptilian remains. The pebbles are chiefly old rocks, black slate, Lydian stone, brown hornstone, white and rose quartz, with an occasional fossil from the mountain limestone, notably joints of the column of Poteriocrinus.

And it would also follow that on these deposits being traced out at sea parallel to the shore the pebbles would cease, the sands would become fine and thin, and ultimately be replaced by clay; moreover, assuming a small river to have brought down

the remains of Megalosaurus, Hylæosaurus, Iguanodon, &c. into a sea tenanted with Pliosaurs, and Ichthyosaurs, and Plesiosaurs, &c., then after depositing these heavy bones and their sand, the fine mud would still be carried out to sea, and with it some of the spoils of the land, as in the case of all river deposits.

The sections at Wicken were simple, being ferrous sands with varying courses of nodules of phosphate of lime and pebbles sometimes united into a bed six feet thick, oftener subdivided by intervening sand into two or three beds of from a few inches to a foot or two thick. Under these and not well separated is a thin band rich in calcareous matter, often making it a hard continuous agglomerate; but it is extremely variable, and sometimes disappears in small isolated concretions. These beds, which were spoken of as the Potton sands and Wicken beds, rest at Wicken partly on the white Upware limestone (usually called Coral Rag, but regarded by the author as not older than the lower part of the Kimeridge clay), and partly on a blue clay with Ammonites serratus, and near the top casts of Nucula and other shells, &c. in phosphate of lime. This blue clay the author regarded as Kimeridge clay, though the Ammonites serratus usually occurs lower in the series. Above the sands is the Galt, the actual junction not seen, though there are cracks in the sands a foot or two wide into which the Galt with its characteristic fossils has been squeezed.

The author followed the Wicken beds to Harrimere, near to which place they form the bed of the river, as a hard dark grey fine sand agglomerate of shells and phosphatic nodules. The species were numerous; those collected are in the Woodwardian Museum. This probably represents the lower phosphate bed at Wicken. Up to the old West Water the phosphatic casts of Lucina, Myacites, Cyprina, Ammonites, &c. occurred plentifully in the bed of the river for two miles before its junction with the Cam. At Stuntney the hill is capped with a thin bed of nodules of phosphate of lime, like those at Wicken with similar

fossils. S.E. of High 'Hill the brown ferrous sands are well seen coming from under the Galt of Soham, which has been bored for 450 feet without being pierced; near the base of the Galt nodules of phosphate of lime and fossils abounded. At Ely these beds are variable. At the Gallows Pits the rock is so calcareous as to split with a crystalline fracture. The old walls of the city are of a fine grit conglomerate with occasional nodules of phosphate of lime, casts of fossils, and bones still to be seen in the blocks; while the Cemetery is on sand, said by the gravedigger to be about 12 feet thick with indurated beds in the middle. In Roswell Hole the conglomerate rock was at the bottom and sand above; here from the rock were obtained about six species of mollusca. At Wilburton a phosphatic band with jaws of Edaphodon occurs near the top of the brown sands. At Haddenham the sands 30 feet thick rest on the upturned and eroded clay [Kimeridge]. In the middle were found three small phosphatic concretions. At Aldreth no concretions were seen. Mr Westrupp states that in dredging the Cam he finds the bottom to be a mixture of gravel and galt between Bottisham sluice and Swaffham sluice; and that at a place near Bottisham sluice called Calves Flat the bed of the river is a hard ferruginous bed with a hard bed below, mixed with gravel and galt. It is highly probable that these are the phosphate beds. Mr Westrupp also states that gravel exists under the whole of Isleham Fen*.

At Downham Market the top of the Kimeridge clay contains small sand concretions and phosphatic casts of shells with green grains in them, resembling in species and preservation those from the sands at Wicken.

At Hunstanton the phosphatic concretions are numerous but chiefly fragments of casts of Ammonites Deshaysii, and about

* Since this paper was read the author has seen this phosphate bed under ferrous sands and graduating into the clay on which it rested at Impington. It appeared to be in situ.

a dozen other mollusca. They occur very near the bottom of the sands, heretofore called the Carstone.

These facts do not demonstrate the position in the geological sequence of the Potton and Wicken beds.

In the south of England the beds between the Kimeridge clay and the galt are

(Lower Green or) North-Down sands*,

Wealden series, ⎫
Purbeck series, ⎬
Portland series, ⎭

and to represent the whole of these there are in this district only the Potton sands. These sands in the middle of England have generally been referred to some *portion* of the series. Thus Smith called them the sand of the Portland rock. Conybeare, who instituted the Ironsand group, supposed the Portland series as well as the Greensand to thin away northward, and so put these deposits into his Ironsands; and Fitton, who instituted the Lower Greensand, supposed the Portland, the Purbeck, and the Weald, to thin off to the north, and threw these beds into the Lower Greensand.

The author then detailed at length the physical characters of the beds between the Kimeridge clay and the Galt in all the English sections, and arrived at the conclusion that the period of elevation indicated by the pebble beds of Potton and Wicken was identical with the period of elevation indicated in the south of England by the Purbeck and Wealden group, the marine equivalents of which would be thin.

And in this district the author supposed the upper part of the clay called Kimeridge to be only the necessary clay representative of beds which to the south are sands.

Under these circumstances it was thought that the old

* The North Downs give the types of the divisions of the so-called Lower Greensand adopted by the Geological Survey.

nomenclature of Cretaceous and Oolite, as divided by Professor Forbes, could not now be sustained. And the author proposed the following physical groups as more true and convenient for English geology.

Mr Seeley's divisions.		Old names.
Cretaceous series.	Chalk Upper Greensand Galt	Cretaceous.
Psammolithic series.	North Down-Sand or Lower Greensand Wealden ⎱ Potton sands and Purbeck ⎰ Wicken beds Portland	Upper Oolite.
Pelolithic series.	Kimeridge Clay Coral Rag and Ampthill Clay Oxford Clay	Middle Oolite.
Oolitic series.	Great Oolite, &c. Inferior Oolite, &c. Lias Trias	Lower Oolite.

In a large number of cases, the fossils, vertebrate and invertebrate, both those with the shells preserved and the casts, had marked affinity with fossils from lower beds. The author endeavoured to account for this by the conditions of physical geography accumulating in one area portions of the fauna from several successive periods, though admitting that some species were probably derived from the denudation of adjacent inferior strata.

Professor Sedgwick made some remarks upon the position and relation of the English greensands, and expiated upon the richness of the deposits of this era near Cambridge and the importance of the Upware fossils.

Professor Liveing asked whether Mr Seeley meant that the Wealden was denuded at Shotover and its neighbourhood to form these beds, and enquired where he supposed this Wealden land to be.

Mr Seeley said that he had not meant to imply that the Wealden land was denuded to form these deposits, but that the animals therein were living in the surrounding seas or were brought down from it by streams into them ; as for the position of the land it was a difficult question, and he was not yet prepared to give a positive opinion, but he believed it to be somewhere to the S.E. of Britain.

Mr Walker (Sidney) asked Mr Seeley whether the presence of Gryphæa dilatata in these beds was not an objection to his theory, and expressed a difference of opinion as to the mode of formation of the phosphatic nodules.

Mr Seeley replied that he thought the presence of a supposed species of oyster a matter of little moment, as the specific distinctions were of such small value.

October 28, 1867.

Professor CHALLIS (VICE-PRESIDENT) in the Chair.

The following were elected Officers of the Society for the ensuing year.

President.

Professor SELWYN.

Vice-Presidents.

Professor HUMPHRY.
Professor STOKES.
Professor CAYLEY.

Treasurer.

Rev. W. M. CAMPION.

Secretaries.

Professor C. C. BABINGTON.
Professor LIVEING.
Rev. T. G. BONNEY.

New Members of the Council.

The Rev. H. W. COOKSON, D.D.
Professor CHALLIS.
Professor MILLER.
Professor ADAMS.

On Skew Surfaces. *By* Professor CAYLEY, F.R.S.

(1) *On the Use of a Camera Lucida Prism in measuring distances and levelling. By* Professor MILLER, F.R.S.

(2) *On a Series of Elevated Sea Terraces on Hampsfell, near Cartmel, Lancashire. By* F. A. PALEY, M.A.
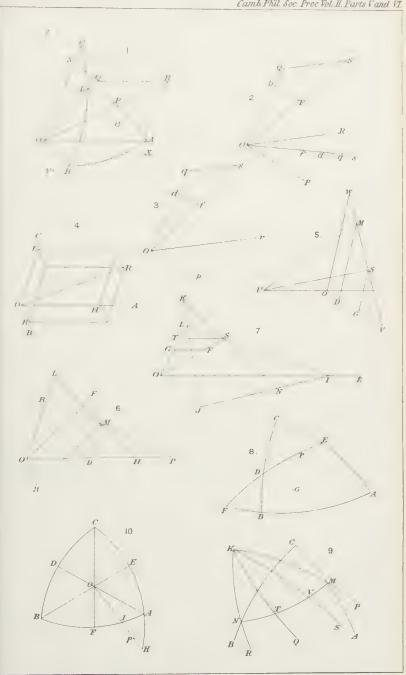
THIS paper gave a description of a remarkable plateau of mountain limestone surmounting the hill, which is something under 800 feet high, that separates Cartmel from the village of Grange, on the Lancaster and Furness Railway. The marks of sea action,—or at least, of aqueous action,—were very distinct over the whole of this plateau of naked rock ; and the resemblance between them and the wave-scored rocks at the level of the present sea is so striking, that the author expressed an opinion that the whole hill had been upheaved within a comparatively recent period. The evidences on which he chiefly relied were, first, a large number of limestone and slate boulders still covering the hill, and evidently the result of glaciers or ice-floes in a glacial sea, and secondly, a well-defined sea terrace, rising with scarcely any break from the present sea below Grange to the crown of the Hampsfell.

A succession of steps or rocky flats on the western side of the hill having the several floors wave-marked in the same manner, and each terminated by a low wall or cliff, were referred by the writer to successive upheavals of the hill subsequent to the deposition of the boulders, which he thought most probably

were dropped when the present hill was submerged a considerable depth below the sea.

Mr Bonney agreed with Mr Paley in considering that the west coast of England and Wales had been submerged beneath the sea at no very distant geological epoch, and gave instances of the occurrence of valleys of marine erosion and sea cliffs in Wales and Cheshire, noticing similar terraces on the great Ormeshead. He, however, doubted whether the scoring and furrowing of the rock could without hesitation be referred to the action of the waves. He had seen many instances of a similar structure in places where glaciers had modified the surface after its upheaval from the sea. This in his opinion had been the case in parts of the Ormeshead, and had certainly been so in many of the Alpine limestone districts, as for example in the Italian Tyrol, on the range of the Parmelan near Annecy, and near the Gemmi Pass in the western Oberland. He believed that the structure was due to the nature of the rock and the way in which it yielded to the agents of subaerial denudation. Although no doubt boulders had often been transported by ice-floes, yet he thought that in the neighbourhood of the Lake District glaciers had also aided.

Mr Paley said that he had very carefully examined the markings on the Fell rocks, and thought that they corresponded so exactly with those on the shore rocks in Morecambe Bay, that the causes which produced them must be identical. As the boulders were usually large and isolated, he thought that they were more probably transported by floes when the country was under water.

Professor Liveing concurred with Mr Bonney. in thinking these surface-markings to be due rather to the 'habit' of the limestone rock, and instanced what, from Mr Paley's description, he thought were similar markings on the inland limestone districts in the Yorkshire Fells.

Mr PRITCHARD (President of the Royal Astronomical Society) presented some copies of a new star-chart and of a chart of the November meteors (1866), and made some remarks in explanation of them, and on the mode of observing these meteors, especially pointing out the importance of counting them, for that by this means the form of the orbit had been roughly inferred. He expected that the shower would take place from about four to six on Thursday morning, Nov. 14th.

November 25, 1867.

The PRESIDENT (Professor SELWYN) in the Chair.

The following new Fellows were elected :

C. J. LAMBERT, B.A., *Pembroke College.*
G. PIRIE, B.A., *Queens' College.*

On Aristophanes. *By the* Rev. W. G. CLARK, M.A. (Public Orator.)

Mr Clark gave a description of the Ravenna MS. of Aristophanes, written in the eleventh century, and traced its history to the library of Urbino. He exhibited photographs of two pages, and described its importance and value as compared with the other MSS. at Venice, Florence, Rome, Modena and Paris.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

> C. TROTTER, M.A., *Trinity*.
> T. W. DANBY, B.A., *Downing*.

The President (Professor Selwyn) presented to the Society a volume containing two tracts on Coinage, one by Nicholas Oresmus; the other by Copernicus; and made a few remarks upon it.

Communications to the Society:

(1) *On the distinction of Subjective and Objective Probability.* By C. J. MONRO, M.A.

(2) *On the existence in the Pterodactyle pelvis of bones like those termed marsupial in the Monotremata, and other indications of mammalian affinities of Pterodactyles.* By H. SEELEY, F.G.S.

The author exhibited a drawing of the femur of a Dimorphodon, and pointed out the close resemblance which it presented in form to that of a mammal; he also exhibited drawings of the pelvis of the same Pterodactyle, and of those of Iguana, Chelidon, and Echidna: shewing that the first had much more affinity with the last than with the other two; he also noticed some peculiarities of the brain of the same; whence he concluded that the mammalian affinities of the Pterodactyle were too marked to be entirely overlooked.

March 2, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

Communication to the Society:

On some phenomena of the weathering of rocks illustrating the value of material denudation as a geological agent. *By* D. T. ANSTED, M.A., F.R.S.

(This paper is printed in the Society's Transactions.)

March 16, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

W. C. GREEN, M.A., *King's.*
E. S. SHUCKBURGH, B.A., *Emmanuel.*
C. A. M. FENNELL, B.A., *Jesus.*
E. HILL, B.A., *St John's.*
E. H. PALMER, B.A., *St John's.*

Communications to the Society:

(1) *On Propositions numerically definite.* *By the late* Professor BOOLE (*communicated by* Professor DE MORGAN).

(This paper is printed in the Society's Transactions.)

(2) *On the Curvæ lineariæ of John Bernoulli.* *By* T. POTTER, M.A.

April 27, 1868.

PROFESSOR HUMPHRY, M.D., VICE-PRESIDENT, in the Chair.

The following new Fellows were elected:

T. WEBSTER, M.A., *Trinity.*
B. ANNINGSON, B.A., *Caius.*

Communication to the Society:

On the Elevation of Mountains by lateral pressure; its causes and amount, with speculations upon the origin of volcanic action. By O. FISHER, M.A.

(This paper is printed in the Society's Transactions.)

May 11, 1868.

PROFESSOR HUMPHRY, M.D., VICE-PRESIDENT, in the Chair.

Communication to the Society:

A description of a new Celestial Globe by Syed Rujjub Allie. By E. H. PALMER, B.A., *St John's College.*

May 25, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

J. F. MOULTON, B.A., *Christ's.*
J. B. HASLAM, B.A., *St John's.*

The Treasurer made his financial statement; his accounts were passed; the Museum account was also audited and passed; and he was authorized to transfer the balance from it to the general account of the Society; and the thanks of the Society were returned to him.

Communications to the Society:

(1) *A supplement to a paper on the discontinuity of arbitrary constants which appear in divergent developments.* By Professor STOKES, M.A., F.R.S.

(This paper is printed in the Society's Transactions.)

(2) *On Transmutation of Species and the Darwinian
    theory of it. By* Professor HUMPHRY, M.D.,
    F.R.S.

This paper was an elaborate review of Mr Darwin's theory,
in which the author made a few remarks on the theological
aspect of the question, and commented upon those parts of it
which he considered to be difficulties in it—such as the fact that
most of the variations in species observed by Mr Darwin had
been under artificial conditions and the absence of transitional
forms.

In the debate which followed, Mr N. GOODMAN and the
PRESIDENT, Professor SELWYN, took part.

October 26, 1868 (Annual General Meeting).

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following were elected Officers of the Society for the
ensuing year.

President.

Rev. Prof. SELWYN.

Vice-Presidents.

Dr HUMPHRY.
Rev. W. G. CLARK.
Prof. CAYLEY.

Treasurer.

Rev. W. M. CAMPION.

Secretaries.

Prof. C. C. BABINGTON.
Prof. LIVEING.
Rev. T. G. BONNEY.

New Members of the Council.

Rev. Dr KENNEDY.

Dr PAGET.

Prof. STOKES.

Communications to the Society:

(1) *On Captain Caron's Zirconia Light;* (2) *On the Chalumeau à platine of MM. Bourbouse et Wiesnegg.* *By* Professor MILLER, F.R.S.

Professor MILLER gave at first a brief sketch of the uses of lime and zirconia, and pointed out the difficulties caused by the lime melting away at a high temperature. The superiority of zirconia, as has been recently demonstrated by Capt. Caron, is that its light is very brilliant, and it is absolutely insoluble under the gas blow-pipe. Zirconia is found in Auvergne, America, and Russia; though it is not very abundant. Professor Miller described an exhibition of this light near Paris, the result of which was very satisfactory.

Professor MILLER then gave a brief description of the *Chalumeau à platine.*

Professor LIVEING spoke of a contrivance by which the inconvenience of the lime melting had been rendered less; also he said that zircon was common in the zircon syenite of the South of Norway, so that he did not expect any difficulty in obtaining the material.

(2) *On the Composition of the Mortar of the old Church of Little Ellingham, Norfolk.* *By* Professor LIVEING.

The object of this communication was to confirm some observations made by Mr Spiller, and laid before the British Association at Norwich this year, to the effect that the hardening of ordinary mortar is chiefly due to the conversion of the lime into

carbonate, and that there is little or no chemical action between the lime and sand, and also to shew that in situations exposed to moisture, such as near the ground, where it is well known that mortar usually sets very hard, the lime in the interior of a thick wall may be almost entirely converted into carbonate.

The old church of Little Ellingham was this year pulled down in consequence of a fire which destroyed the roof and otherwise damaged the building, and the mortar analysed was taken from the south wall while in process of demolition, at a spot about a foot above ground and about the same distance from either face of the wall, which was about two feet thick. This mortar was quite unaffected by the fire. The walls were of flint, and Mr Healey of Bradford, the architect of the new church, considers them to have been built about the year 1400 A.D. An inferior limit to the age of the building is fixed by extant records relating to the insertion of a particular window in 1469, which window remained until the fire this year, so that the mortar may be assumed to have been 400 years old at the least. The following are the results of the analysis :—

$CaCO_3$, &c. soluble in HCl...... 27·49
Sharp siliceous sand .... 72·51
                                  _____
                                  100·00

The part soluble in HCl gave in 100 parts

CO_2 .... 39·21
SiO_2 .... ·83
SO_3 .... 1·53
CaO.................. 52·64
MyO .... ·95
Na_2O .... ·05
Fe_2O_3, Al_2O_3 ...... ·82
H_2O .... 5·10
                        _____
                        101·13

The quantity of soluble silica is not more than might be expected in any specimen of lime and cannot be attributed to the action of the sand on the lime. The quantity of BC^2 is equivalent to 49·9 of CaO, and the remainder of the lime appeared to be partly in the form of sulphate, and probably partly in the form of $CaCO^3$, CaH^2O^2, since treatment of the mortar with water failed to extract any appreciable quantity of hydrate. The water is more than would be combined in the sulphates, so that perhaps a small quantity of the $CaCO^3$ was in the hydrated form, such as is precipitated spontaneously from a solution of CaH^2O^2 in syrup.

November 9, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

H. JACKSON, M.A., *Trinity*.
T. DALE, M.A., *Trinity*.
G. H. DARWIN, B.A., *Trinity*.

In noticing the presents, the President called attention to a beautiful engraving of the nebula in the sword-handle of Orion, presented to the Society by Lord Rosse.

The Astronomer Royal made some remarks on the engraving, calling attention to the fact that it was the first in which dark stood for dark sky and light for the brighter parts.

Communications to the Society:

(1) *On the factorial resolution of* $x^{2n} - 2 \cos na + \dfrac{1}{x^{2n}}$.

By the ASTRONOMER ROYAL.

This was a proof of the well-known resolution by the use of ordinary algebra, without the aid of imaginary quantities, against the use of which as a mode of education the author of the paper spoke strongly.

Professor Challis said that though impossible quantities might not be very good instruments of education, they were, in higher investigations, absolutely necessary, and true in the strictest sense.

Professor Cayley believing that the theory of impossible quantities was absolutely true, did not wish it to be excluded from the University studies or placed in a secondary position.

Professor Adams, admitting that imaginary quantities might be used without proper logic, considered that though undiscovered there must be a logic discoverable, and thought the Astronomer Royal's proof of the Theorem not the natural proof, since it was so complicated. He also held that the proof of imaginary quantities could be, and, in some cases had been rendered strictly logical.

(2) *On some Porismatic Problems. By* W. K. CLIFFORD.

The PROBLEM :—To draw a polygon of a given number of sides, all whose vertices shall lie on one given conic, and all whose sides shall touch another given conic : is either not possible at all, or possible in an infinity of ways. This remark, originally made by Poncelet, has been shewn by Professor Cayley to depend in a very beautiful manner upon the theory of elliptic functions ; and in this way he has proved that an analogous theorem holds good wherever a (2, 2) correspondence exists : that is to say, whenever two things are so related that to every position of either there correspond two positions of the other. Two points x, y for instance, in a conic U, which are connected by the relation that the line xy touches a second conic, V, have a correspondence of this kind : for if the point x be taken arbitrarily, two tangents can be drawn from it to V, determining two positions of y : and conversely, the point y being fixed determines two positions of x. The theorem is then that in a (2, 2) correspondence there is either no closed cycle of a given order, or an infinite number. In the present com-

munication I propose first to prove this result by the method of correspondence alone, and then to extend the proof to higher orders of correspondence.

In a $(2, 2)$ correspondence there are $4 (= 2 + 2)$ united points, that is to say, four points each of which coincides with one of its correspondents. In fact, if two numbers x and y are connected by an equation of the second degree in each of them, then when we make x and y coincide, there results an equation of the fourth degree (Chasles, *Comptes Rendus*, 1864). I call these united points the points a. Each point a has one of its correspondents coinciding with it ; it has also another correspondent b. Each point b again has another correspondent c, and so on. There are also four points α, each of which is such that its two correspondents coincide in a point β. For let q be a correspondent of p and r a correspondent of q; then the relation between p and r is a $(2, 2)$ correspondence (since to each position of p there are two positions of r and *vice versa*), and therefore has four united points, viz. the points β. Each of these points β has another correspondent γ, and so on. We have thus two series of points, $abcd \ldots \alpha\beta\gamma\delta \ldots$ each letter indicating a set of four generally distinct points.

Let us now endeavour to obtain a closed cycle of an odd order : for distinctness' sake we will try to draw a pentagon inscribed in one conic, U, and circumscribed to another, V. Start with a point x on the outer ; pass to one of its correspondents, y ; y has another correspondent, z ; from z we go to u, from u to v, from v to w. If now w were the same point as x, we should have succeeded in our object. But the relation between w and x is a $(2, 2)$ correspondence, for we might have started from x in either of two directions. The united points of this correspondence should therefore apparently give solutions of our problem.

But these united points are no other than the four points c. For starting with one of these, we get the cycle $cbaabc$, which

is a sufficient solution of the correspondence problem last enunciated. But it is *not* a solution of the original problem : for the series will go on *cbaabcde*...and not repeat itself, so that the points *cbaab* do not form a *proper* in-and-circumscribed pentagon. Thus the problem is in general impossible. If however there is any proper solution, the equation of the fourth degree (which determines the improper solutions) will have more than four roots, and will therefore be identically satisfied by any number whatever; so that whatever point x we start with, the point w will come to coincide with it.

Precisely similar reasoning is applicable to the cycles of an even order. Thus e.g. for a quadrilateral we get the four improper solutions $\gamma\beta\alpha\beta$ got by starting from the points γ. I pass to the consideration of correspondences of higher orders.

In an (r, r) correspondence there are

$$2r \text{ united points } a \text{ ;}$$

their remaining correspondents form

$$2r(r-1) \text{ points } b \text{ ;}$$

to these again correspond

$$2r(r-1)^2 \text{ points } c, \text{ and so on.}$$

Similarly there are

$$2r(r-1) \text{ points } \alpha,$$

each of which is such that two of its correspondents coincide ; viz. these are

$$2r(r-1) \text{ points } \beta,$$

to which also correspond

$$2r(r-1)^2 \text{ points } \gamma, \text{ and so on.}$$

Now if we attempt to form a closed cycle of the nth order, we shall be led to a correspondence

$$\{r \cdot (r-1)^{n-1}, \ r \cdot (r-1)^{n-1}\},$$

which has $2r \cdot (r-1)^{n-1}$ united points. From this number we shall have to subtract the number of improper solutions as given

by our previous reasoning; thus we shall find

$(n = 2m + 1)$, $\{2r \ (r - 1)^{2m} - 2r \ (r - 1)^{m}\}$ proper solutions.

$(n = 2m)$, $\{2r \ (r - 1)^{2m-1} - 2r \ (r - 1)^{m}\}$ proper solutions.

For example, the problem to inscribe on a conic a triangle whose side shall touch a given curve of the third class admits of twelve proper and twelve improper solutions. If the number of proper solutions exceeds this number, the problem becomes porismatic: that is to say, there is an infinite number of solutions.

<div align="right">W. K. C.</div>

Professor Cayley spoke of the importance of Mr Clifford's remarks.

The President exhibited some photographs of the transit of Mercury, taken at Ely, and described the manner in which they were taken.

The Astronomer Royal said that when a small planet approaches the edge of the sun's disk, a black line is observed to shoot out from it, and rapidly thicken out. The cause of this is irradiation, by which the sun appears larger and the planet smaller than they ought, and the true contact with the sun's rim takes place when the black line is first seen; he thought the appearance simply ocular, but wanted further evidence.

Professor Cayley exhibited a model of a certain developable surface.

<div align="center">

November 23, 1868.

</div>

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

New Fellow elected:

<div align="center">

A. MARSHALL, M.A., *St John's.*

</div>

Communications to the Society:

(1) *On the comparatively recent date of the period when the works of Greek authors were first committed to writing.* *By* C. A. M. FENNELL, M.A.

*The following is the Author's abstract of this Paper,
which is printed in the Society's Transactions.*

Full statement " That among the Greeks Prose Literature
was first committed to writing not earlier than the Persian
wars, that is, in Ionia not before 510 B.C., in the rest of Greece
not before 490 B.C., and that Metrical literature was first indited
several years later, say 450 B.C."

Importance of the subject.

Mr Paley's opinion quoted.

Appeal against allowing prejudices on the subject to in-
fluence argument.

The theory based on 1st, strong negative evidence of clas-
sical authors; 2nd, inferences from general history; 3rd, in-
spection of inscriptions; 4th, indications given by the voca-
bulary.

Examination of evidences given by Herodotus and Thucy-
dides.

Heraclitus the earliest writer mentioned expressly by an
extant classical author.

For the Homeric papers, see Mr Paley's paper.

Passages from Dr Thirlwall's History.

Evidences of metrical authors.

History of the progress of the art consistent with the Author's
theory. The limits of its application.

Reasons for its tardy development.

The character of the alphabet.

Testimony of inscriptions.

Effect of Persian wars.

Reasons for the priority of Ionian prose literature.

Suggestions as to how the change from the use of memory
to that of writing was effected.

Discussion of the reported existence of libraries. Attic lite-
rature in Solon's time.
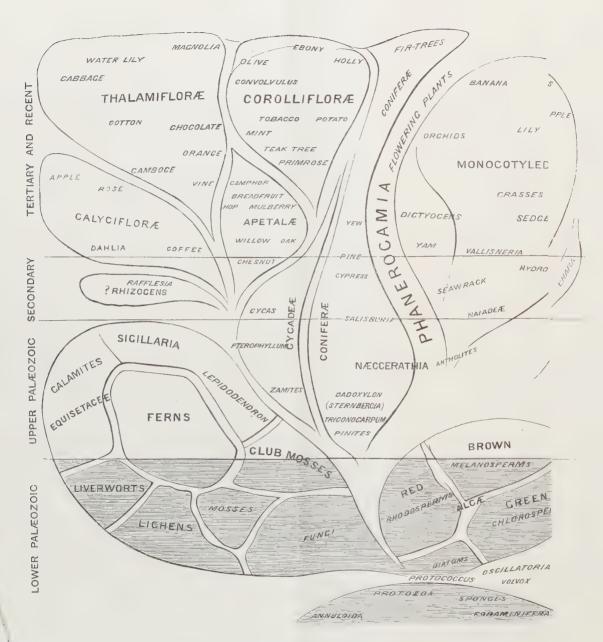
Diagram illustrating Mr SALTER's paper on 'Plant life,' p. 125.

Beginnings of Prose—Genealogies and Theogonies—Philosophical treatises—οἱ λόγοι.

Origin of the common opinion.

Registration.

Legends ascribing remote antiquity to writing.

Indications given by vocabulary and phraseology.

Concluding remarks on the Greek education and character.

(2) *A note on the resolution of* $x^n + \dfrac{1}{x^n} - 2 \cos n\alpha$ *into*

factors.

Monday, *February* 8, 1869.

The President (Professor Selwyn, D.D.) in the Chair.

Communications to the Society :

(1) *On the Succession of Plant Life upon the Earth.*
By J. W. Salter, F.G.S., &c.

In the course of a work on Fossil Botany, I found that I could exhibit in a tabular view a scheme of Plant-life which should combine a natural arrangement of the Vegetable orders with their Geological succession : and I have the pleasure to lay this map before the Society this evening.

I have adopted a prismatic order for the geological colours[1], because that order truly expresses the gradual and harmonious transition of one epoch into another. And since 1848, when I laid this scheme of colouring before the British Association, I have seen no arrangement of geological colours which better expresses the sequence of formations, and their relation one to another. There are no hard lines in the spectrum, and there are no sharp lines of demarcation in the geological history. What is wanting at one place is filled up in another ; and, in the succession of plants, as well as of animals, the progress of

[1] The colouring is omitted in the printed copy of the diagram.

discovery only tends to link yet more closely the rocks of each successive period one with the other.

And if, in placing the whole of the flowerless plants of the lower part of the series, I appear to sin against the obvious facts, that most of the *Cryptogamic* orders continue to the present day, I beg it will be understood, that by this arrangement I have only attempted to indicate a *maximum* for each succeeding epoch ; and by no means would assert, that the higher tribes had not their roots further back in time than we have yet discovered them. And still less, of course, to assert, contrary to the fact, that branches of the Cryptogamic group do not persist till now,—the humble descendants of what was once the reigning dynasty.

I do not think it necessary to quote Dana, whose pregnant comparison of successive geological epochs with human dynasties must be fresh in the memory of every geological student. The commencement of one ruling caste in the history of men, as of lower organic groups, during the culmination of another, is so natural and familiar to us, that it only needed the genius of that accomplished author to clothe it in appropriate language,—and then to become an accepted axiom in natural history. I therefore make no apology for restricting the Cryptogamia to the *Palœozoic*; or the Flowering plants to the *Neozoic* strata. And I wish the tabular scheme to speak for itself, with only the following remarks :

1. We have, in the early stratified rocks, evidence of the existence of banks of *seaweed* and *nullipore*, only by means of the beds of Graphite and stratified limestone, which occur all over Canada. The origin of these is not doubtful to the chemist, and Mr Sterry Hunt has already claimed them as of Vegetable origin.

2. In the Lower Cambrian rocks of Sedgwick, traces only of low *Algœ* have been found (*Corallines* and other calcareous forms): and in the Silurian, fronds of seaweed, apparently as

highly developed as the *Fucus* of our own coasts, are numerous enough.

3. At the close of the Silurian period, the great tribe of *Club-mosses* made its appearance, in an anomalous form; and thenceforward through the Devonian and Carboniferous eras developed enormously in variety, and of size unknown in modern times. With these, during the same period, *Ferns* in great variety, *Equisetaceæ* of giant size, (*Calamites*) and *Sigillaria*,—a tree, which, related to all the higher *Cryptogamia*, rose almost to a level in structure with the *Cycad*. And here, in the *Coal-measures, Cycadeæ* and *Coniferous* trees began to abound. The rank of these amongst flowering plants has been questioned, but no one who considers their manner of growth, imperfect inflorescence, and naked seeds, can doubt that they are inferior to the Oak and the Magnolia,—the rose and the lily of modern times.

4. The botany of the secondary period is less known than it should be. But *Cycads* and *Coniferæ* appear to have risen to their maximum therein; while traces only of a few aquatic *Monocotyledons* are found, to justify us in arranging with these, a few doubtful plants which seem to belong to this order in the higher Coal-measures, but the nature of which is still *sub judice*. I refer to such plants as *Antholithes* and *Pothocites* in the coal; and to the *Naiadaceæ* (pond-weeds, &c.) found sparingly in Oolitic strata.

5. The Cretaceous period abounded in Coniferæ, but included in its upper portion true *Dicotyledons* and *Monocotyledons*, yet the *Iguanodon* must have browsed chiefly on marshy ferns and marsh-loving firs and cypresses, while the Tertiary flora was being ushered in.

6. The Tertiary flora, though capable of much subdivision, so much resembles part of our own forest fauna, that it is needless to speak more fully of it in this short paper. With a distribution of forms wholly unlike what obtains at the present day,

the fruits and stems and leaves found in the London clay and the leaf-beds of the Isle of Wight shew us *Palms* and *Screw-pines, Cassia, Myrtle,* Leguminous plants in great variety, and very few *Compositæ*: yet the Mediterranean flora and that of the *Miocene* may be well compared: and the oaks and willows, tulip trees, *Rhamni,* and plane trees of America, mingled with Palms and Cinnamons from the tropics, and Proteaceæ from the Cape, are all to be found in Miocene strata; and they abound in the lower Miocene which is so perfectly developed in Switzerland, and which has received the attention of a dozen good European botanists (*Heer, Raulin,* &c. &c.).

There has thus been apparently a true " succession of Plant-life " on the globe. And should any one object that the data are too few to furnish a basis for argument, let it be answered, that the data as far as known, agree with those drawn from the much more perfect series of buried animals. And further, that the progress of all true discovery has been this : that the known data should be accepted and reasoned on, until better turn up. And that an hypothesis may be greatly useful, even if founded on scanty data, provided we include nothing that is uncertain—and use it, as Sir H. de la Beche used to say—only as a peg to hang facts upon.

(2) *A sketch diagram of the Passage of the Brachio-poda to the Bivalves and the Bivalve to the Univalve.* *By* Mr SALTER, F.G.S.

(3) *Note on Crotalocrinus rugosus, a remarkable Crinoid in the Woodwardian Museum.* *By* Mr SALTER, F.G.S.

(2) and (3) are printed in the Society's Transactions.

Professor BABINGTON in expressing his approbation of the map drawn up by Mr Salter, spoke of the great difficulty of assigning fossil plants to their order and genera, which difficulty was perhaps less in the coal measures.

Professor Humphry complimented Mr Salter upon his clear and able exposition of the relation of the bivalves and univalves, as exemplifying the great principles of modification which may be observed in nature, shewing how with immense variety in detail there is little in principle. With regard to the water vascular system, he thought there was no reason why the organs should not be respiratory as well as excretory; these functions being not unfrequently blended in the lower animals.

Mr Seeley did not admit Prof. M'Coy's claim to the relation of the muscles and valves; denied that the palpæ in the Lamellibranchs corresponded in function with the spiral coils of the Brachiopods. These and the external covering of the brachiopod correspond much more nearly with those of the polyzoa; reference, he held, should rather be made to the radiata than the lamellibranchs. As a case in point, he instanced perforations common in the brachiopod shells. He thought the brachiopod diverged almost as far from the ordinary molluscan type, as do the echinodermata from the orders to which they are most nearly related.

Mr Salter in combating Mr Seeley's remarks, said he preferred to seek analogies in those orders which had most resemblance rather than in those which had no external resemblance.

February 22, 1869.

The President (Professor Selwyn, D.D.), in the Chair.

Communications to the Society:

(1) *On the bird-like characters of the Brain and Metatarsus in the Pterodactylus from the Cambridge Greensand. By* Mr H. Seeley, F.G.S.

This was a note upon a specimen of the skull of Pterodactylus, which had been obtained by Mr Walker (Sidney Sussex College), and placed by him in Mr Seeley's hands. The latter,

on examining it, found that the cast of the brain was remark-
ably clear and perfect. The characteristics disclosed by this
cast, when developed, were very remarkable; it did not resemble
those of the pisces or reptilia, but presented affinities to some
extent with the birds, and still more with the lowest orders of
mammalia, especially with the ornithorhynchus. This discovery
corroborated the theory advanced some time since by Mr Seeley,
that the Pterodactyles were much more nearly allied to the birds
than to the reptiles.

(2) *Note on the Pterodactylus macrurus (Seeley) a new
    species from the Purbeck Limestone, indicated by
    caudal vertebræ five inches long.* By Mr H.
    SEELEY, F.G.S.

This specimen was remarkable for its extraordinary size, the
largest that has been obtained from the Cambridge chloritic
marl being one and a half inches long.

(3) *Note on the thinning away to the westward in the Isle
    of Purbeck of the Wealden and Lower Greensand
    strata.* By Mr H. SEELEY, F.G.S.

The various sections made by Mr Seeley, in company with
Mr Sedley Taylor, of Trinity College, were carefully described
and tabulated.

(4) *On the coincidence of the Moon's periods of rotation
    on her axis and synodical revolution round the
    earth as an electro-magnetic phenomenon.* By
    Mr POTTER.

The whole discussion of the moon's rotation on her axis
commencing with Newton's original solution, supposes that the
periods of rotation on her axis and of her synodical revolution
round the earth, were *equal* or *nearly equal*, whilst her mass

was in the fluid state, and that her figure became ellipsoidal with the longer axis towards the earth under the actions of the centrifugal force from rotation, and the ablatitious force of the earth's attraction on the fluid mass.

The librations of the moon commencing with the diurnal libration discovered by Galileo, and the libration in longitude discovered by Hevelius, as well as the libration in latitude were explained by Newton on the above supposition. Cassini, the celebrated French Astronomer Royal, discovered the remarkable property of the lunar motions that the nodes of her equator coincide with the nodes of her orbit, and that a plane through her centre parallel to the plane of the ecliptic lies between the planes of her equator and her orbit, so that the poles of the ecliptic, her orbit, and her equator are in the same great circle, but the two latter on opposite sides of the first. He concluded that the inclination of the lunar equator to the ecliptic was 2° 30′.

Mayer, in the middle of the last century, found the inclination of the moon's equator to the ecliptic to be 1° 45′. Lalande later found it to be 2° 9′, but recent observations confirm Mayer's result.

These are called apparent librations, but Newton discussed the existence of a real libration or oscillation of the longer axis of the moon's figure about its mean place. D'Alembert, Lagrange, and Laplace applied refined analytical methods to this problem, but the conclusion of Professor Grant of Glasgow, Fellow of the Royal Astronomical Society, in a note in his excellent work, the History of Astronomy, has the following: "It is natural enough, indeed, to suppose that the illustrious author of the Principia did not feel any anxiety to repudiate the original equality of the motions of rotation and revolution —a relation which, although perhaps difficult to explain by the doctrine of chances, becomes very interesting and suggestive when it is considered as the result of Supreme Intelligence."

The whole of the discussions of these eminent men involved this supposition of the *equality* or *near equality* of the periods of rotation and revolution whilst the moon's figure was forming, as the bases of their solutions, and having only the theory of gravitation as the foundation of their methods, they had no option left to them.

Since about the year 1820, we have gradually become acquainted with other powerful forces in nature. Œrsted's discovery that a magnetic needle sets itself at right angles to a circuit over or under which it is placed when a galvanic current passes along that circuit, was quickly followed by others still more surprising, and the science of electro-magnetism in its various branches, has in less than 50 years from its birth grown to be a most extensive as well as a most important science. The branch which we call magneto-electricity arose afterwards, and it is to the forces of magneto-electricity that I shall refer for explanation of the lunar phenomena before related.

I must be allowed to diverge from the simple course of procedure to remind the meeting that bodies may be now arranged in two classes which we call magnetic and diamagnetic bodies. The class of magnetic bodies comprehends those which are attracted towards the poles of powerful magnets, and the class of diamagnetic bodies comprehends all the others, whether neutral or repelled from the poles of powerful magnets. The first class comprehends many metals, as iron, nickel, cobalt, manganese, chromium, &c., crown-glass, potassium, sodium, many salts and oxides of the magnetic metals, as well as magnetic minerals, and oxygen gas. The second class contains bismuth, phosphorus, antimony, heavy glass, or silicated borate of lead, zinc, &c., flint-glass, mercury, lead, silver, copper, gold, &c., nitrogen gas.

Hot air is diamagnetic with respect to cold air, and hence the peculiar action of powerful magnets on flames.

The repulsion of the most diamagnetic substances, as bis-

muth, phospherus, &c. from the poles of magnets is a very weak force compared with the attraction of the more magnetic metals. We recognize also that magnetic bodies have the property of *polarity*, which does not exist in diamagnetic bodies.

The discovery of Dr Faraday that oxygen gas is magnetic was a most important step, and we may now conclude that the earth's magnetism resides very greatly, if not entirely, in the oxygen gas of the atmosphere, for the greater part of the earthy materials of the earth's crust are diamagnetic substances, and the abundant mineral per-oxide of iron is still small in quantity compared with the others, and only feebly magnetic.

It is not easy to find the place of oxygen gas amongst the magnetic substances from the small weight contained in such glass globes as we can use in experiments, but the black or protoxide of iron is very magnetic. In the original experiment exhibited by Dr Faraday, when he made known his discovery at the meeting of the Royal Institution, at which I was present, the weight of the oxygen gas in the globe of perhaps 2 inches diameter would be less than $1\frac{1}{2}$ grains, and the flint-glass of the globe was feebly diamagnetic yet though surrounded by atmospheric air of which about $\frac{1}{5}$th of the volume is oxygen gas; and notwithstanding the resistance to motion which such a globe would experience, and its inertia, yet it was evidently steadily though slowly attracted to the poles of the magnet. Since that time, I, every session of my lectures, exhibited the same result without a single case of failure, to my experimental class of Natural Philosophy in University College, London.

If we consider that the weight of the oxygen gas of the atmosphere is more than $\frac{1}{5}$th of the weight of the barometric column or more than the weight of 6 inches depth of mercury covering the whole surface of the earth or considerably more than that of 10 inches depth of iron, we may conclude that the earth's magnetism lies very greatly if not entirely in the

oxygen gas of the atmosphere, of which each particle is a small magnet having its north and south pole. Now substances which are magnetic lose this property at high temperatures, and the late Professor Barlow, of Woolwich, in his treatise on magnetism, states that *white hot* iron ceases altogether to be magnetic. So that, if the magnetic metals exist in large quantities in the interior of the earth, they will cease to be magnetic, from the high temperature of the internal heat. From these considerations, it is probable that the earth owes its claim to be a magnetic body to the oxygen of the atmosphere, and the coldest parts on the earth's surface are the localities of the magnetic poles, the definition of the magnetic pole being the place where the dipping needle takes the vertical position; the north magnetic pole, in round numbers, being in 70° North latitude, and 100° West longitude.

According to Professor Barlow and Mr Charles Bonnycastle, the investigation of the magnetic meridians in various places shews that they only converge ultimately towards some places within the arctic and antarctic circles.

The moon having no visible atmosphere we have no like reasons to offer for her being a magnetic body, and the volcanic appearance of her surface has all the appearance of the diamagnetic substances, and the interior still fluid, as shewn by the occasional volcanic eruptions which are noticed, will also from the great heat be diamagnetic.

The relations of the earth and moon are therefore those of a smaller diamagnetic body in the presence of a larger magnetic body, and the phenomena of magneto-electricity will be effective between them, since the magnetic force acts freely through a vacuum as well as through dense bodies.

About the year 1825, M. Arago made the discovery that if a circular disk of copper be set rotating rapidly parallel to and under a bar magnet which is suspended so as to be free to move parallel to the disc, then the magnet goes into rapid rotation

in the same direction as the disc. Sir John Herschel and Mr Babbage [1] soon afterwards tried the converse experiment of causing the magnet to rotate parallel to a suspended disc, when the disc commenced to rotate in the same direction as the magnet. Now in both these cases if the suspended body were prevented from rotating, a reaction tending to destroy its rotation would take place upon the rotating body. The science of magneto-electricity being unknown at the time of these discoveries, they were inexplicable from previously known properties of magnetic and non-magnetic bodies. We now know [2] that when a diamagnetic body is set in motion near a magnet, that electrical currents arise in the moving body and by Œrsted's discovery, the magnet tends to set itself at right angles to the current thus formed, and so goes in Arago's experiment into rotation; and if prevented doing so, it reacts on the rotating body to destroy its rotation.

The converse experiment to the above is a very impressive one and more directly applying to our present subject. It is the immediate and sudden destruction of rotatory motion in an angular diamagnetic body rotating near the poles of a powerful electro-magnet, when the current is formed. The sudden destruction of the momentum in a heavy rotating square prism of copper in such a case surprises us when first seen, from there being no evident force to produce it.

Now in the case of the magnetic earth and diamagnetic moon, the same forces but in different degrees must arise, and if the moon ever possessed or received a rotatory motion, *relative to the earth*, it would gradually but surely cease.

The moon's absolute rotatory motion in her synodic period forms a no rotatory motion with respect to the earth; which is the point involved in the magneto-electric phenomena. Some years ago there was an active controversy as to whether

[1] See their paper read before the Royal Society, 16th June, 1825.
[2] See Faraday's paper read before the Royal Society, 24th Nov. 1831.

the moon had really any rotatory motion, but the controversy ceased when attention was called to the necessary distinctions between relative and absolute motions.

As the same law of no rotatory motion of the satellites of Jupiter with respect to their primary is believed to exist, we are led to conclude that Jupiter is a magnetic body, and his satellites are diamagnetic bodies.

March 8, 1869.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The Meeting was held, by permission of the Museums and Lecture Rooms Syndicate, in the Anatomical Lecture Room, New Museums.

The following new Fellows were elected :

> Rev. G. HENSLOW, M.A., *Christ's.*
> RICHARD C. JEBB, M.A., *Trinity.*
> LINNÆUS CUMMING, M.A., *Trinity.*

On the generation of clouds by actinic action, and the reaction of such clouds upon light. By Professor TYNDALL, F.R.S.

Professor Tyndall commenced by referring to the distinction drawn by Fichte between the processes used by two different classes of minds to arrive at clearness of religious belief, the logical and intuitive methods. These methods, he said, had a no less important bearing upon the observation of external nature. To *know* nature's operations required a long and careful scientific training, but to *see* them was in the power of men of culture and imagination. Perhaps an image of natural processes placed before even persons destitute of the scientific training would be interesting, and might woo some of those who had hitherto been *seers* to be *knowers* of nature.

He then proceeded to perform an experiment shewing new actions effected under new conditions, rays of light producing chemical action. Into a long glass tube carefully exhausted, a small quantity of air had been allowed to pass through tubes containing (1) cotton wool, (2) caustic potash and marble, (3) sulphuric acid and glass, (4) nitrite of amyl, with the vapour of which last substance the air in the experimenting tube was loaded.

A beam of electric light was then passed along the tube, and a beautiful cloud was at once formed, gradually extending from the end nearest the lamp towards the other. The tube was then reversed, and the same phenomenon produced at the other end. What, asked the lecturer, is the vapour in which this change has been produced? A collection of molecules, each molecule being built up of nineteen atoms; the waves of light beating against these invisible molecules have broken them up and re-arranged them in such a form that they become visible.

In this case the vapour of the substance itself was directly exposed to the action of the light; other substances require to be introduced into the experimenting tube in connexion with vapours which aid as it were the process of decomposition. Benzole vapour for example is unaffected, but if mixed with air passed through aqueous nitric acid, with which it has a *tendency* to combine, it is precipitated at once on the beam of light. This is an illustration of one of the commonest of nature's operations, vegetation: the carbonic acid and the chlorophyll are side by side in leaves, ready to unite but unable to do so of themselves; the ray of sunshine falling on the leaves consummates the union, gives the green colour to the leaves, and sets oxygen free into the air.

The action of which an example has been given might be made very slow if the vapour tested were sufficiently attenuated; the growth of the visible particles might be very slow. What should we expect under these circumstances?

The lecturer here paused to exhibit Newton's experiment of the decomposition of a beam of white light into its constituent coloured rays, and referring to a diagram shewed that the energy of the waves which produced the sensation of red was many times as great as that of those which produced blue. Then naturally we should expect that the blue vibrations (using this term for the sake of brevity), having least energy, would be soonest stopped by particles floating in the medium through which the light passed. That this was so in fact the next experiment clearly shewed : the cloud produced in the vapour of nitrite of butyl (though not so clearly produced as on some previous occasions), was distinctly visible to those who were near the table, and was of a blue or violet shade. The light emitted from the cloud was moreover perfectly polarized, as was tested by the interposition between it and the eye of a crystal of tourmaline. The next experiment, owing to some defect in the apparatus, the preparation of which had been necessarily hurried, did not succeed, but was described by the Professor as the excitation of a real cloud within the tube containing an actinic cloud and the reproduction on a small scale of the azure of the sky. It was, as he described it, taking a piece of the sky and producing in limited space all the phenomena of cloud light and polarization which are produced by the sun light and the clouds of heaven.

To shew the exceeding minuteness of the particles to which the blue colour of the sky is due, Professor Tyndall described the difficulty of getting rid of the residue of vapour in the experimenting tube, a difficulty which had more than once led him to false conclusions until the experiment which the residuary vapour vitiated had been repeated. The last experiment was an examination of the vapour by means of polarized light, in which the cloud, distinctly visible in one direction, was entirely lost to sight when the polarizing prism had been revolved through an angle of forty degrees.

But the most interesting part of the lecture was the con-
clusion; the former parts were not absolutely novel, but the
latter had never been announced before. These clouds and the
similarity of their appearance to the tails of comets, have led
Prof. Tyndall to form a Theory of Comets, which he enun-
ciated on this occasion for the first time. The theory, which
at any rate collects and accounts for observed facts, is as
follows:—

1. The theory is that a comet is composed of vapour decom-
posable by solar light, the visible head and tail being an actinic
cloud resulting from such decomposition. The texture of an
actinic cloud is exactly that of a comet.

2. The tail, according to this theory, is not projected mat-
ter, but matter precipitated on the solar beams traversing the
cometary atmosphere. It can be proved by experiment, that
this precipitation may occur with comparative slowness along
the beam, or that its consummation may be practically momen-
tary throughout the whole length of the beam. The amazing
rapidity of the development of the tail would be thus accounted
for without invoking any motion of translation save that of the
solar beams.

3. As the comet wheels round its perihelion, the tail is not
composed throughout of the same matter, but of new matter pre-
cipitated on the solar beams which cross the cometary atmo-
sphere in new directions. The enormous whirling of the tail is
thus accounted for without invoking a motion of translation.

4. The tail is always turned from the sun for this reason.
Two antagonistic powers are brought to bear upon the cometary
vapour: the one an actinic power tending to produce precipita-
tion; the other a calorific power tending to effect vaporization.
Where the former prevails we have the cometary cloud; where
the latter prevails we have transparent cometary vapour. As a
matter of fact, the sun emits the two powers whose agency is
here invoked. There is nothing hypothetical in the assumption

of their existence. That precipitation should occur behind the head of the comet or in the space occupied by the head's shadow, it is only necessary to assume that the sun's calorific rays are absorbed more copiously by the head and nucleus than the actinic rays. This augments the relative superiority of the actinic rays behind the head, and enables them to bring down the cloud which constitutes the comet's tail.

5. The old tail, as it ceases to be screened by the nucleus, is dissipated by the solar heat; but its dissipation is not instantaneous. The tail leans toward that portion of space last quitted by the comet, a fact of observation being thus accounted for.

6. In the struggle for mastery of the two classes of rays, a temporary advantage, owing to variations of density or some other causes, may be gained by the actinic rays even in parts of the cometary atmosphere which are unscreened by the nucleus. Occasional lateral streamers and the apparent emission of feeble tails towards the sun would be thus accounted for.

7. The shrinking of the head in the vicinity of the sun is caused by the beating against it of the calorific waves, which dissipate its attenuated fringe, and cause its apparent contraction.

At the conclusion of the lecture, which was listened to with the greatest attention by a large audience, the President (Rev. Professor Selwyn) conveyed the thanks of the Society to the lecturer, who briefly acknowledged the compliment, and expressed his pleasure in being able to appear before the Society.

NOTICE.

Vol. XI. Part II. of the Transactions of the Society is ready, and every Fellow of the Society who has paid his subscription for the current year may have one copy gratis upon sending a written application to Professor LIVEING, Secretary.

Fellows requiring the Part to be sent to them must transmit postage stamps to the value of 6d. The copies of those who do not send stamps will be left at the Porter's Lodge at the New Museums; but no copy will be issued until the Meeting of the Society next after the receipt of the application for the same.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

[*Reprinted, April* 1898.]

The President (Professor Selwyn, D.D.) in the Chair.

The President presented a set of photographs of the planet Mercury, during its transit over the Sun, Nov. 5th, 1868, taken at Ely, which were of unusual interest as being the only photographs taken in England on that occasion.

The thanks of the Society were returned to him, and also to Professor Challis for a copy of his work on *Principles of Mathematics and Physics*.

The Treasurer (Mr Campion) presented his accounts, and congratulated the Society on its improved and improving financial condition.—The thanks of the Society, proposed by Dr Paget and seconded by the Master of Trinity, were returned to the Treasurer.

Communications made to the Society:

A new interpretation of a disputed passage in Thucydides iv. 30. *By* Professor Kennedy, D.D.

The main purpose of this paper was to shew that the word αὐτοῦ before ἐσπέμπειν was to be taken as a genitive pronoun and referred to τὸν σῖτον, meaning that the General was sending in a supply of corn for a number smaller than itself, viz. than was represented by the quantity of corn (which was delivered in rations for each man).

Mr Paley, while admitting the value of the paper, doubted whether the collocation of αὐτοῦ after instead of before τὸν σῖτον quite suited Professor Kennedy's interpretation.

Mr Munro agreed with Professor Kennedy; saying that αὐτοῦ in the sense of "there" appeared to him quite otiose and unmeaning, and thought that the collocation did not present any difficulty.

Mr W. C. Green remarked upon a difficulty in the use of ποιεῖσθαι, and rather agreed with Mr Paley about the position of αὐτοῦ.

Mr Fennell defended the reading αὐτούς, thinking it to mean that the Lacedæmonians were trying to make their number appear smaller than it was, in order that the Athenians might think little of their advantage.

Mr Hammond saw no difficulty so far as the collocation of αὐτοῦ went, but rather preferred the reading αὐτοῖς ; and shewed reasons why the Spartans would be very anxious to make peace at that period of the war ; and expressed an opinion that the provisions introduced had been such as would not keep.

Some conversation also took place, in which the above, the Master of Trinity, the Provost of King's, and the President took part.

The President also mentioned that, if he remembered right, Bauer had hinted at this meaning of αὐτοῦ, and thought the Professor's translation of αὐτοῦ rather harsh.

April 26, 1869.

Communications made to the Society :

(1) *Note on passages in Aristotle's Meteorologica and in Sir J. Herschel's Astronomy relating to the sight of faint objects, and on some passages of Ancient Poets relating to the lost Pleiad. By* the President, Professor Selwyn, D.D.

The fact that there seemed to be, as noticed by the above authorities, a lesser sensibility of that part of the retina in the line of direct vision, the author thought might be due to the greater use of that part in the ordinary work of the eye. He

then passed on to make some remarks on the Pleiads mentioned by the Greek Poets sometimes as six, sometimes as seven, and concluded that the uncertainty of the number was due to the above fact.

Professor KENNEDY made some remarks on the derivations of the name Pleiad.

Professor CHALLIS thought that one of the stars might be a variable one.

Mr POTTER made a few remarks on the appearances presented by the blood vessels in the eye.

Professor MILLER said these could well be seen on waking suddenly.

Mr TROTTER mentioned that, as shewn by some experiments lately made at Heidelberg, the focus centralis took a longer time to receive the impression of objects, but retained it longer.

Dr PAGET thought the matter required further experiments, and mentioned ways in which it would be desirable to test it.

Some further conversation occurred, in which the MASTER OF TRINITY, Mr W. C. GREEN, and the above took part.

(2) On new and general Equations for the Equilibrium of Flexible Surfaces. By RICHARD POTTER, A.M.

[Abstract.]

In a paper on the " curvæ lineariæ " of John Bernoulli which the author read in May 1868 before the Society, he stated that he believed the equations which Poisson had investigated in his " Mémoire sur les surfaces élastiques " read before the French Institute on the 1st August 1814, might be brought to comprehend all the ordinary cases to which we wish to apply them, by restoring a factor which M. Poisson had struck out. Soon after reading the paper he came to a different conclusion, and undertook to investigate the equations for the equilibrium of flexible surfaces, from mechanical rather than mathematical

considerations, and found the problem to be much simpler than had been generally supposed, and that the three necessary and sufficient equations for the equilibrium of an element of the surface could be obtained in a very simple manner.

By the principles of statical science, when three forces are in equilibrium at a point they must be in the same plane, because the resultant of any two must be equal in magnitude and opposite in direction to the third force. We know therefore that when two opposing tensions and an external force are in equilibrium at an elementary *area* of a flexible surface, the same rule must hold good; and it also must hold good if the tensions transmitted in any manner through a sheet are equivalent to resultant tensions acting in different directions through the element of the surface, for each set of resultant tensions and their corresponding portions of the external forces. From these considerations we learn, that in all the ordinary problems of the equilibrium of flexible surfaces, of regular forms and symmetrical positions, where the external forces arise from gravity or the pressure of fluids, the tensions will act along the lines of curvature of the sheet, since it is only for points taken in succession along such lines, at right angles to each other at each point, that consecutive normals to the surface meet, and the conditions can be satisfied.

If s and s' are arcs of the lines along which the resultant tensions act, measured from any fixed points; ds and ds' elements of these arcs at right angles to each other, forming the sides of an elementary area $ds . ds'$ upon the sheet, of which the thickness at this element is τ, and density ρ; then $\rho\tau ds . ds'$ is the mass of the element. Let X, Y, Z be the external accelerating forces, acting parallel to the axes of coordinates respectively upon the elementary mass. Let T be the tension due to a unit of breadth, acting in the direction of s upon the element, T' that acting in the direction of s'; with dx, dy, dz the components of ds in the axes respectively; and dx', dy', dz' those of ds'.

Then the component tensions acting on the element in the directions of the axes respectively, will be

$$Tds' \cdot \frac{dx}{ds}, \qquad Tds' \cdot \frac{dy}{ds}, \qquad Tds' \cdot \frac{dz}{ds}$$

for Tds' the tension acting on the element in the direction of s; and

$$T'ds \cdot \frac{dx'}{ds'}, \qquad T'ds \cdot \frac{dy'}{ds'}, \qquad T'ds \cdot \frac{dz'}{ds'}$$

those for $T'ds$.

Now the variations of these in passing from one side of the element to the opposite side will be the only internal forces entering the equations of equilibrium; and the three necessary and sufficient equations for the equilibrium of the element under the action of the external and internal forces become as follows,

$$X\tau\rho \cdot ds \cdot ds' + \frac{d\left(Tds' \cdot \frac{dx}{ds}\right)}{ds} \cdot ds + \frac{d\left(T'ds \cdot \frac{dx'}{ds'}\right)}{ds'} \cdot ds' = 0 \ldots\ldots (1),$$

$$Y\tau\rho \cdot ds \cdot ds' + \frac{d\left(Tds' \cdot \frac{dy}{ds}\right)}{ds} \cdot ds + \frac{d\left(T'ds \cdot \frac{dy'}{ds'}\right)}{ds'} \cdot ds' = 0 \ldots\ldots (2),$$

$$Z\tau\rho \cdot ds \cdot ds' + \frac{d\left(Tds' \cdot \frac{dz}{ds}\right)}{ds} \cdot ds + \frac{d\left(T'ds \cdot \frac{dz'}{ds'}\right)}{ds} \cdot ds' = 0 \ldots\ldots (3),$$

which are applicable generally to the cases of the equilibrium of flexible surfaces.

When the external force is a normal pressure N on a unit of area, such as the pressure of a fluid, and R, R' are the principal radii of curvature at the element, then by resolving in the direction of the normal and performing the differentiations, we obtain the well known formula $N = \dfrac{T}{R} + \dfrac{T'}{R'}$ whether T and T' be constant or variable since the coefficients of $\dfrac{dT}{ds}$ and $\dfrac{dT'}{ds'}$ disappear.

The case of the catenary curve for a heavy rectangular sheet suspended by two of its opposite sides from two parallel horizontal straight lines is easily and concisely discussed from these equations.

The case of the " curva velaria " of James Bernoulli is easily investigated by means of them.

The case of the form which a piece of bladder tied over the circular aperture in the receiver of an air-pump, when a portion of air is withdrawn from the interior, depends on the difference of the pressures on the internal and external surfaces, and the nature of the surface to bear them. When the form the bladder takes is known then the tensions at given points are found from the formulæ.

The phenomena of capillary attraction being those of flexible surfaces, the results which the author had obtained by longer methods of procedure are more concisely investigated by means of these equations.

<div align="center">May 10, 1869.</div>

The President (Professor SELWYN, D.D.) in the Chair.
New Fellows elected :

>   RICHARD SHILLETO, M.A., *St Peter's College.*
>   THOMAS M'KENNY HUGHES, M.A., *Trinity College.*

Communications made to the Society :

(1) *On a Group of Figures with archaic inscriptions on one of the Leake Vases in the Fitzwilliam Museum. By* Mr PALEY.

This vase has been described as " the invasion of Troy by Hercules." Mr Paley thought it perhaps rather the conflict of Hercules with the Amazons. The interest of it was that it bore in some respect on the Homeric question ; for it alluded to a legend mentioned by Pindar but not by the Homer whose works we now possess. Mr Paley first spoke of the purpose of vases in tombs, doubtless to contain food for the use of

the ghost; he then described a fragment of a lecythus from Smyrna—after which he quoted the allusions to the Legend of Hercules and his attack on Troy from Pindar. This invasion is here represented in the vase, names being given to the figures—Andromache being one of them. The Leake vase came from Vulci, Etruria: it is an amphora about 18 inches high, belonging rather to the archaic than the fine art period: it represents Hercules and Telamon fighting against six women, armed with circular shields and broad bladed spears; the shield bearing devices: character of ornamentation, Assyrian. Six of the nine names are written backwards; E, O, are used for H, Ω; H is used as rough breathing; the forms of other letters are peculiar. He also entered at length into other points in the Legend.

(2) *Is volition a function of material forces only? and can a Planet exist as a habitable world for ever? By* Mr RÖHRS.

The argument in this paper was of too abstruse a nature to admit of abstracting.

Mr MOULTON and Mr CLIFFORD made some remarks objecting to the theory advanced in the paper.

(3) *A series of comparative views of the Solar Disc, and of Planetary Configurations. By the* President, Professor SELWYN, D.D.

Professor SELWYN briefly described the mode of taking heliographs of the sun, carried on during five years at Ely. On the back of each photograph the planetary configuration was inscribed, with a view to ascertain the connexion between the planet and solar spots.

Professor CHALLIS thought the investigation important, and mentioned one or two well-known facts concerning the solar spots.

October 25, 1869 (Annual General Meeting).

Professor CAYLEY in the Chair.

The following were elected Officers of the Society:

President.

Professor CAYLEY.

Vice-Presidents.

Professor SELWYN.
Rev. W. G. CLARK.
Mr TODHUNTER.

Treasurer.

Rev. W. M. CAMPION.

Secretaries.

Professor C. C. BABINGTON.
Professor LIVEING.
Rev. T. G. BONNEY.

New Members of the Council.

THE MASTER OF GONVILLE AND CAIUS.
Dr P. W. LATHAM.
Rev. J. C. W. ELLIS.
Mr P. T. MAIN.

Communications made to the Society:

(1) *On some supposed Pholas Burrows in Carboni-ferous Limestone Rocks. By* T. G. BONNEY, B.D.

The following are the results of the author's observations on burrows on the Great and Little Orme's Head:—(1) They are clearly the result of the action, mechanical, chemical, or

both, of some living agent. Many of them are in positions
where rain or wind cannot reach them, run almost vertically
up into the rock. and are practically impervious to water at
the highest point. (2) They are rarest on surfaces much ex-
posed to prevailing winds, or where the rock approaches to
a grit. (3) They usually occur on boulders, or projecting
rocks, at no great distance from the surface of the soil; in
not a few cases the turf had actually grown into them.
(4) The axis of the burrow usually is not at right angles
to the surface of the rock ; often is only inclined at a slight
angle to it, so that the burrow commences as a channel (if
this be not a natural depression utilised), and sinks gradually
into the rock. Frequently it is driven into some slight pro-
minence, as though the burrowing animal had first sheltered
itself under the lee of this, and then gradually worked its
way deeper into the rock. (5) The burrows are very fre-
quently curved. Sometimes the tangents to the axis at the two
extremities, if produced to meet, would include an angle not
very much greater than 90°. (6) *Helices*, especially *H. ad-
spersa*, are generally abundant in the neighbourhood of these
burrows ; empty shells are common in them, and in the
freshest, smoothest, and most unweathered of them, he always
found a living *Helix*. (7) The constriction in the upper part
of the burrow characteristic of perfect Pholas excavations, is
generally wanting, and though the burrow sometimes con-
tracts towards the mouth, this is often not quite regular in
form ; so that a *Helix* that would exactly fit the end would
be able to quit the burrow. At least he believes this to have
been the case with all that he examined. The ends, also, of
the burrows are, he thinks, generally rather flatter than is
usual with Pholas holes.

From the above considerations, and especially from the
position of some cavities in the roof of a narrow crack in the rock,
the conclusion is, he thinks, irresistible, that these are not the

weathered burrows of departed *Pholades*, but have been and are being hollowed out by *Helices*, the principal, if not the only agent, being *H. adspersa*. A specimen of the burrows was exhibited.

Mr SEELEY described *Helices* which he had seen at Charlton (Kent), which hollowed themselves small depressions in cracks in the chalk. Also Pholas burrows which he had seen in soft limestone in various parts of the south of England. These were very unlike the specimen handed round. He thought, however, that although Mr Bonney had admitted it, the burrows need not have been effaced by submersion, giving instances of their preservation.

Mr BONNEY replied that he thought that the Carboniferous limestone if immersed must have been weathered, and pointed out that most of the instances adduced by Mr Seeley were in harder rocks.

Mr FISHER said that the holes not being vertical to the surface was fatal to their being Pholades, but quoted the opinion of Mr Pengelly, who thought they were made by some marine mollusks.

(2) *Tidal phenomena investigated according to the laws of fluid motion, taking into account fluid friction. By* Mr RÖHRS.

Professor ADAMS made some remarks on the difficulty of the subject.

The ASTRONOMER ROYAL made a remark upon Laplace's theory of the tide.

Professor CHALLIS stated the mode in which he should wish the problem attempted.

(3) Mr FISHER exhibited a flint implement which he had found on a heap of gravel which was stated to have been dug at Chesterton.

Professor CAYLEY, and during part of the evening Professor CHALLIS, in the Chair.

New Fellow elected:

   Rev. E. K. GREEN, M.A., *St John's College.*

Communications made to the Society:

(1) *On a certain Sextic Torse.* *By* Professor CAYLEY.

(2) *The Bedawin of Sinai and their traditions.* *By*
  Mr E. H. PALMER.

The Arabs were much less wanderers than was generally supposed in Europe, seldom moving except from winter to summer camps; though their ideas of right of property differed from our own, their character was far better than usually believed. They have no history because no *nationality*, only a *clanship*; therefore there is rarely any concerted action. There are about 4000 grown males in the Towarah or Sinai Bedawin. They are not aboriginal, but came with the Mohammedan conquerors; the aboriginals were an Aramæan race, to be found perhaps among the Jebaliyeh or mountain tribe. Mr Palmer then mentioned the various divisions of the tribes. The Sheikh was rather an arbitrator than an adjudicator, negotiating business for the tribe; he, however, interposed to make equitable arrangement in cases of debt or theft, which latter is rare. The Agyd is a military officer, hereditary and only bearing office in time of war. The mode of marriage was then described. The bridegroom calls on the father, and a price is arranged, after which great rejoicings take place; there is then a ceremony of betrothal by pressing a piece of herb wrapped in a turban before the Khatib (the bride is not consulted). On her return the bridegroom's mantle is suddenly

thrown over her by the Khatib, who pronounces the bridegroom's name. Various ceremonies are gone through for three days, and then she is taken home to her husband's house. Death— The corpse is washed, a little bag of corn placed beside it, and it is then lowered into the grave; certain prayers being said over it, the grave is filled, and a feast is held. The women bewail the dead with loud cries.

The Arabs are not an irreligious race, though less demonstrative than other eastern races in their observances. Their prayers were described by Mr Palmer, who repeated one of them. They believe that the monks of the Convent can bring rain. They believe in a general resurrection, when the world shall melt; the good will rise with their hands above their heads, the wicked with their hands by their sides; vultures come, the former can drive them away, but the latter have their eyes pecked out. They believe that snakes may be seen fighting for a stone, which, if secured, gives immunity from snake bites. Several curious superstitions about the Convent were related. Arab tradition, though influenced by monkish legend, contains independent evidence of the Exodus. This is the legend of the departure from Egypt : Moses and Pharaoh having quarrelled, the former fled with the Israelites; the Egyptians followed and were drowned much as described in the Bible, save that the Hammam Pharown was supposed to be formed by Pharaoh's drowning struggles. The name of Moses was attached to many spots in the peninsula. Among others, the Arabs point out a rock as struck by Moses, and severed by his sword because it impeded his path. The rock at Rephedim is an invention of the monks, but in Wady Feiran they shew a rock from which they say Moses obtained water, when the Israelites were athirst. Another rock is said to have had water drawn from it. The primitive dwellings are called Mosquito houses, said to have been raised to protect the Israelites from a plague of mosquitoes. They point

out some ruins near Hazeroth as belonging to a caravan who afterwards were lost in the Tih, and never heard of again. There was other corroborative evidence to shew that this last legend referred to the Israelites.

November 22, 1869.

The President (Professor CAYLEY) in the Chair.
New Fellow elected:

S. S. LEWIS, B.A., *Corpus Christi College.*

Communications made to the Society:

(1) *On the Degeneration of Curves. By* W. K. CLIF-FORD, M.A., *Trinity College.*

(2) *On a Machine for solving Equations. By* Mr J. C. W. ELLIS.

(3) *The Reptiles of the Kimeridge Clay of Cambridge-shire. By* Mr H. SEELEY, F.G.S.

To which was added a note on an animal of the Pterodactyle kind from the Wealden, which was larger than any known land animal.

February 21, 1870.

The President (Professor CAYLEY) in the Chair.
Communications made to the Society :—

(1) *On the Antiquity of some of our familiar Agri-cultural Terms. By* F. A. PALEY, M.A.

The author pointed out that the digamma sound was still retained in English, as for example in our numeral "one;" so that this form was more ancient than that of $\epsilon\hat{\iota}s$, which was once digammated. He then called attention to the tendency of language to reveal primitive forms, and to the fact that agricultural life was favourable to the preservation of old words. Probably many old Aryan words still survive among the peasantry.

Niebuhr observed that agricultural terms were generally of common origin in both Greek and Latin, though the Oscan war terms were without representatives in Greek. In English it would be noticed that while the generic names of animals were usually of Saxon origin, the words denoting their application were of Latin or Greek derivation; thus the words for cooked flesh were from the Norman. Words when not generic, but particular and descriptive, generally appear to have representatives in the classical languages. Mr Paley then gave a large number of instances of these rules, concluding with some remarks on the antiquity of " plough " and " harrow." The former, he thought, was connected with the root of $\pi\lambda\acute{e}\omega$, and he noticed the frequent metaphorical use in poetry, as of a ship " ploughing the water." " Harrow " he connected with the root of $\chi\alpha\rho\acute{a}\sigma\sigma\omega$, and the word " harass."

Professor SELWYN asked whether *to ear* was still used in England for *to plough*, as in Chaucer, Shakespeare, and the Bible?

Mr LUMBY objected to $\check{\alpha}\chi\upsilon\rho\sigma\nu$ being referred to the same root as " chaff," as Mr Paley had done. He agreed with him in rejecting the popular derivation of " gallop," and mentioned a confirmation of the derivation of the word " bull " (*bubulus*) in " bugle," which is used by Sir John Maundeville for " bull; " afterwards for a musical instrument made from the horns.

Mr W. C. GREEN thought " plough " might be from the same root as $\pi\lambda\acute{\eta}\sigma\sigma\omega$.

(2) Proof that every Rational Equation has a Root. By W. K. CLIFFORD, B.A., Trinity College.

[Abstract.]

The proof contained in the present communication depends on the determination of a quadratic factor of the rational integral expression

$$x^{2s} + a_1 x^{2s-1} + a_2 x^{2s-2} + \ldots + a_{2s}.$$

On dividing this expression by $x^2 + p_1 x + p_2$, we obtain by the ordinary algebraic rules a remainder of the form $M_{2s-1} x + N_{2s}$, where M_{2s-1} and N_{2s} are functions of p_1 and p_2 whose weights are $2s-1$ and $2s$ respectively, and which may accordingly be written in the forms

$$M_{2s-1} = b_{2s-1} + p_2 b_{2s-3} + \ldots\ldots + p_2^{s-1} b_1,$$
$$N_{2s} = c_{2s} \quad + p_2 c_{2s-2} + \ldots\ldots + p_2^{s},$$

where the b, c are of an order in p_1 indicated by their suffixes. On writing down (by Professor Sylvester's Dialytic method) the result of eliminating p_2 between these equations, it is at once apparent that this resultant is of the order $s(2s-1)$. Thus the determination of a quadratic factor of an expression of degree $2s$ is reduced to the solution of an equation of order $s(2s-1)$. But this number is *one degree more odd* than the original number $2s$; that is to say, if the number $2s$ is 2^g multiplied by an odd number, then $s(2s-1)$ is 2^{g-1} multiplied by an odd number. Hence by a repetition of this process we shall ultimately arrive at an equation of odd order, which, as is well known, must have a real root. By then retracing our steps the existence of a quadratic factor of the original expression is demonstrated.

(3)   *On the Space-Theory of Matter.*   By W. K. Clifford, B.A., *Trinity College.*

[*Abstract.*]

Riemann has shewn that as there are different kinds of lines and surfaces, so there are different kinds of space of three dimensions; and that we can only find out by experience to which of these kinds the space in which we live belongs. In particular, the axioms of plane geometry are true within the limits of experiment on the surface of a sheet of paper, and yet we know that the sheet is really covered with a number

of small ridges and furrows, upon which (the total curvature not being zero) these axioms are not true. Similarly, he says, although the axioms of solid geometry are true within the limits of experiment for finite portions of our space, yet we have no reason to conclude that they are true for very small portions: and if any help can be got thereby for the explanation of physical phenomena, we may have reason to conclude that they are not true for very small portions of space.

I wish here to indicate a manner in which these speculations may be applied to the investigation of physical phenomena. I hold in fact

(1) That small portions of space *are* in fact of a nature analogous to little hills on a surface which is on the average flat; namely, that the ordinary laws of geometry are not valid in them.

(2) That this property of being curved or distorted is continually being passed on from one portion of space to another after the manner of a wave.

(3) That this variation of the curvature of space is what really happens in that phenomenon which we call the *motion of matter*, whether ponderable or etherial.

(4) That in the physical world nothing else takes place but this variation, subject (possibly) to the law of continuity.

I am endeavouring in a general way to explain the laws of double refraction on this hypothesis, but have not yet arrived at any results sufficiently decisive to be communicated.

March 7, 1870.

The President (Professor CAYLEY) in the Chair.

New Fellows elected:

W. G. ADAMS, M.A., *St John's College.*
A. T. CHAPMAN, M.A., *Emmanuel College.*

Communications made to the Society:

(1) *On the Centrosurface of an Ellipsoid.*
By Prof. CAYLEY.

The centrosurface of any given surface is the locus of the centres of curvature of the given surface—or say it is the locus of the intersections of consecutive normals, (the normals which intersect the normal at any particular point of the surface being those at the consecutive points along the two curves of curvature respectively which pass through the point on the surface). The terms, *normal, centre of curvature, curve of curvature,* may be understood in their ordinary sense or in the generalised sense referring to the case where the Absolute (instead of being the imaginary circle at infinity) is any quadric surface whatever: viz. the normal at any point of a surface is here the line joining the point with the pole of the tangent plane in respect of the quadric surface called the Absolute; and of course the centre of curvature and curve of curvature refer to the normal as just defined.

The question of the centrosurface of a quadric surface has been considered in the two points of view, viz. 1°, when the terms "normal" &c. are used in the ordinary sense, and the equation of the quadric surface (assumed to be an ellipsoid) is taken to be $\frac{X^2}{a^2} + \frac{Y^2}{b^2} + \frac{Z^2}{c^2} = 1$. 2°, when the Absolute is the surface $X^2 + Y^2 + Z^2 + W^2 = 0$, and the equation of the quadric surface is taken to be $\alpha X^2 + \beta Y^2 + \gamma Z^2 + \delta W^2 = 0$:—in the first of them by Salmon, *Quart. Math. Jour.* t. II. pp. 217–222 (1858), and in the second by Clebsch, *Crelle,* t. 62, pp. 64–107 (1863). See also Salmon's *Solid Geometry,* 2nd Ed. 1865, pp. 143, 402, &c. In the present memoir, as shewn by the title, the quadric surface is taken to be an ellipsoid; and the question is considered exclusively from the first point of view: the theory is further developed in various respects, and in particular as

regards the nodal curve upon the centrosurface : the distinction of real and imaginary is of course attended to. The new results suitably modified would be applicable to the theory treated from the second point of view : but I do not on the present occasion attempt so to present them.

(2) *On the correct expressions for the resistance which bodies experience, whilst moving in gases and liquids; with a description of the verifying experiments. By* RICHARD POTTER, A.M.

[*Abstract.*]

The mathematical discussion of the resistance to motion which bodies experience whilst moving in fluid media has remained hitherto in a very imperfect state, although it received great attention from Sir Isaac Newton. In the second Book of the *Principia* he discusses the resistance when bodies move in elastic and non-elastic media.

Canton having in 1762 proved the liquids to be elastic as well as the gases, though they are subject to different laws of compressibility, the consideration which Newton adopted for elastic fluids we know now to apply to all fluids. The Problem VII. Prop. XXXV. of Book II. of the *Principia*, the author held to be the problem still to be solved by the instrument of the modern analysis which was undeveloped in Newton's days. It is enunciated as follows :

" Si medium Rarum ex particulis quàm minimis quiescentibus, æqualibus, et ad æquales ab invicem distantias liberè dispositis constet : invenire resistentiam Globi in hoc medio uniformiter progredientis."

This involves the method which the author followed in his paper on Hydrodynamics published in the *Philosophical Magazine* for March, 1851, before he knew that Newton had considered elastic fluids to be constituted of distinct molecules in

discussing such problems. We are led to consider the fluids as consisting of congeries of heavy very small nuclei surrounded each by imponderable existences, as caloric, electricity, &c., and the volume of each molecule depending chiefly, if not entirely, on the calorific atmosphere. When the whole volume of a fluid body is made up of such molecules we must attribute a cube of space to each molecule, since the cube is the only regular figure which will fill all space symmetrically without vacancies, and the faces of this cube are the areas on which the pressures are to be taken which are transmitted to the nucleus through the means of its elastic atmosphere. These cubes will be of the same volume throughout surfaces of equal pressure and temperature, but will continually vary in volume along directions of variable pressure and temperature. An imaginary vertical cylindrical column of the atmosphere supposed of homogeneous constitution will form an illustration of this; the attributed cubes of the molecules being of equal volume in horizontal strata, but varying imperceptibly in the vertical direction.

If at any point in a fluid δs is the perpendicular distance of the center from the faces of the cube, and therefore $2\delta s$ is the length of the edges, and the distance of the centers of contiguous nuclei; then $4\delta s^2$ is the area of each face of the cube, and $8\delta s^3$ is the volume of the cube. If m is the mass of the nucleus and also of the cube, and ρ the density of the fluid at this point and also the average density of the cube, we have $m = \rho . 8\delta s^3$. Let p be the pressure on a unit of area at the given point, and therefore the pressure on each face of the cube is $p . 4\delta s^2$ in equilibrium. When s is the space measured from any fixed point along the line of variable pressure at right angles to the surface of equal pressure where the pressure is p; then if p becomes p' in surfaces of equal pressure similarly situated at a distance $s \pm 2\delta s$, we have, by Taylor's theorem,

$$\pm \delta p = p' - p = \pm \frac{dp}{ds} . \frac{2\delta s}{1} + \frac{d^2p}{ds^2} . \frac{4\delta s^2}{1 . 2} \pm \frac{d^3p}{ds^3} . \frac{8\delta s^3}{1 . 2 . 3} + \&c.$$

When there is not equilibrium, the internal moving force arising from variation of pressure on the opposite faces of any cube will be $\pm \delta p \cdot 4\delta s^2$. If R is the resultant *external* accelerating force acting on the nucleus, mR is the moving force; and if v is the velocity relative to the neighbouring particles of the fluid, we have $m \dfrac{dv}{dt} = mv \dfrac{dv}{ds}$ the effective force acting on the molecule. Then by D'Alembert's principle we have

$$mR \pm \delta p \cdot 4\delta s^2 - mv \frac{dv}{ds} = 0,$$

and $m = 8\rho \delta s^3$;

therefore
$$R \pm \frac{\delta p}{\delta s} \cdot \frac{1}{2\rho} - v \frac{dv}{ds} = 0.$$

When $R = 0$ and the problem is such that we may stop at the first term in the expansion of δp, as in the problem of resistances, by substituting and reducing we have

$$\pm \frac{1}{\rho} \cdot \frac{dp}{ds} - v \frac{dv}{ds} = 0,$$

where we must put $\rho = \dfrac{p}{\kappa}$ for the gases, and $\rho = \dfrac{\rho_{,}}{1 - cp}$ for the liquids by Canton's law. This expression differs from that found by the ordinary method only in having the double sign, which has never been hitherto found on the methods of Euler, Laplace, Lagrange, or of all those who following them more or less implicitly have considered fluids as continuous homogeneous bodies; so that they have never been able to find more than the *front* resistance which bodies experience whilst moving through fluids. This consideration leads us to conclude that such methods are essentially defective and erroneous, and that the science of hydrodynamics requires the recognition of the atomic or molecular constitution of fluids.

Let $p_{,}$ be the pressure on a unit of area when the relative velocity v is nothing, then for gases $\rho = \dfrac{p}{\kappa}$, and integrating we find

$$p = p_, \epsilon^{\pm \frac{v^2}{2\kappa}} = p_, \left(1 \pm \frac{v^2}{2\kappa} + \frac{v^4}{1 \cdot 2 \cdot 4\kappa^2} \pm \frac{v^6}{1 \cdot 2 \cdot 3 \cdot 8\kappa^3} + \&c. \right),$$

and therefore

$$\pm (p - p_,) = p_, \left(\frac{v^2}{2\kappa} \pm \frac{v^4}{1 \cdot 2 \cdot 4\kappa^2} + \frac{v^6}{1 \cdot 2 \cdot 3 \cdot 8\kappa^3} \pm \&c. \right),$$

where the + sign refers to the front and the − sign to the back of the plane unit of area, when there is a relative velocity v either by considering the moving fluid as impinging on the area at rest, or the fluid at rest whilst the plane area moves through it in a direction perpendicular to its surface.

When v is not large, stopping at the first term of the expansion, we have the resisting force

for the front of the plane $p - p_, = p_, \dfrac{v^2}{2\kappa} = \dfrac{\rho}{2} v^2,$

for the back of the plane $p_, - p = p_, \dfrac{v^2}{2\kappa} = \dfrac{\rho}{2} v^2,$

or the front and back resistances are nearly equal for slow velocities, but the whole, being the sum of the two resistances, is the double of that hitherto investigated. They act as a force pushing the body back in front and pulling it back behind, as by a force of suction.

Substituting for $\rho = \dfrac{\rho_,}{1 - cp}$ for liquids by Canton's law and integrating we find

$$\pm (p - p_,) = \frac{\rho_,}{1 - \dfrac{c}{2}(p + p_,)} \cdot \frac{v^2}{2},$$

and neglecting the term having the multiplier c (the compressibility being always exceedingly small in liquids), we have the same expressions for the front and back resistances as for the gases with slow velocities. When the moving body is at or near the surface of the liquid there are circumstances requiring attention which can be investigated as follows: let z be the depth of the liquid which produces the pressure $p_,$, so that

$p_{,} = g\rho z$, and h the height a body must fall to acquire the velocity v, or let $v^2 = 2gh$, then substituting these in the expressions for the front and back resistances, we have for the front

$$p = p_{,} + \frac{\rho}{2} v^2 = g\rho \, (z + h),$$

and the pressure on the plane area in front increases with the depth z, and with h or v^2 directly. For the back we have

$$p = p_{,} - \frac{\rho}{2} v^2 = g\rho \, (z - h),$$

and the pressure on the back of the plane is nothing for $z = h$, and also for all less values of z. This can be seen in the easy experiment of moving a flat rod, or even a walking stick briskly through the water, when to a certain depth it will be seen that air follows the rear of the rod, and to a certain depth there is no pressure of water on the rear of the rod. This experiment, in accordance with theory, shows that experiments for the resistances which bodies such as ships and boats moving at the surface of water experience must be tried at the surface, and not through the body of the fluid.

Having investigated the above expressions for the front and back resistances of a plane moving in a direction perpendicular to its surface, the rest of the investigations for the resistances of bodies of different forms are similar to those used in elementary analytical treatises on hydrostatics and hydrodynamics. Thus, if a small plane area moves obliquely in a fluid, let θ be the angle between the direction of motion and the perpendicular to the plane, then the velocity perpendicular to the plane is $v \cos \theta$, and the perpendicular resistance varies as $v^2 \cos^2 \theta$, and this resolved in the direction of the motion is $v^2 \cos^3 \theta$. When a solid of revolution moves in the direction of its axis of revolution, taken for the axis of x, in a fluid, we have the area of an elementary ring equal to $2\pi y ds$ if s is the arc of the curve by whose revolution the front part of

the surface is generated with the ordinate y, and $\cos\theta$ becomes $\dfrac{dy}{ds}$.

If we take s' for the arc of the hind part of the body, we have the area of the elementary ring of the same radius y equal to $2\pi y ds'$, and $\cos\theta'$ for the hind part equals $\dfrac{dy}{ds'}$. If we put μ and μ' for the coefficients of v^2 at the front and back respectively, we have the resistances experienced by the elementary rings of radius y equal to

$$2\pi\mu v^2 y \,\frac{dy^3}{ds^3}\, ds = 2\pi\mu v^2 y \,\frac{dy^2}{ds^2}\, dy$$

for the front, and

$$2\pi\mu' v^2 y \,\frac{dy^2}{ds'^2}\, dy$$

for the back of the body.

Integrating the sum of these, we have the resistance experienced by the solid of revolution moving in the direction of the axis equal to

$$2\pi v^2 \left\{ \mu \int y \,\frac{dy^2}{ds^2}\, dy + \mu' \int y \,\frac{dy^2}{ds'^2}\, dy \right\}.$$

This expression becomes simplified when the front and back surfaces are the same, or when one of them is plane, and gives the following results.

The previous investigations show that both in gases and liquids for moderate velocities the values of μ and μ' are nearly equal and each equal to $\dfrac{\rho}{2}$ nearly. Taking $\mu = \mu' = \dfrac{\rho}{2}$, we find the following:

Ex. 1. To find the resistance experienced by a circular disc or short cylinder, moving in a fluid with a velocity v in a direction perpendicular to its plane.

When b is the radius of the disc, then the whole resistance front and back $= \pi\rho v^2 b^2$.

Ex. 2. To find the whole resistance experienced by a sphere of a radius b moving in a fluid with a relative velocity v.

We find the resistance $= \dfrac{1}{2}\pi\rho v^2 b^2$, which is half that found for a plane circular disc in the previous example considered as a great circle of the sphere. This resistance experienced by a sphere is, however, the double of that found in the treatises on hydrodynamics hitherto published.

Ex. 3. To find the resistance experienced by a hemisphere of a radius b moving in a fluid in a direction perpendicular to its plane surface, but with either the plane or curved surface first.

We find the resistance $= \dfrac{3}{4}\pi\rho v^2 b^2$ in each case, which is half as much again as that experienced by the whole sphere.

Ex. 4. To find the resistance experienced by a spheroid moving in the direction of its axis of revolution in a fluid.

Let a be the axis about which the revolution takes place in the formation of the surface, and b the other axis of the generating ellipse.

Then the resistance

$$= \pi\rho v^2 \frac{b^4}{a^2 - b^2}\left\{ \frac{a^2}{a^2 - b^2}\log_e\left(\frac{a^2}{b^2}\right) - 1 \right\},$$

from which the results of Examples 1 and 2 as particular cases may be obtained by expansions for prolate and oblate spheroids.

Ex. 5. To find the resistance experienced by a segment of a right cylinder on an elliptic base moving in a fluid in the direction of the major axis of the ellipse.

If a be the axis of the ellipse in the direction of the motion,

b the other axis of the ellipse, and $e^2 = 1 - \dfrac{b^2}{a^2}$, also f equal the height of the cylinder, then the resistance on the whole surface

$$= 2\rho v^2 fb \cdot \frac{1-e^2}{e^2} \left\{ \frac{1}{e\sqrt{1-e^2}} \sin^{-1} e - 1 \right\}.$$

Ex. 6. To find the resistance experienced by a right cone moving in the direction of its axis with either the apex or the base first.

Let a be the height and b the radius of the base of the cone, then the resistance on both surfaces

$$= \frac{1}{2} \pi \rho v^2 b^2 \left\{ \frac{b^2}{a^2 + b^2} + 1 \right\},$$

which diminishes as a increases, and becomes $\dfrac{3}{4} \pi \rho v^2 b^2$ when $a = b$, which is the same as for the hemisphere.

Ex. 7. To find the resistance experienced by a double cone moving in the direction of its axis in a fluid.

Let a be the height of each of the cones, b the radius of their common base.

Then the resistance $= \pi \rho v^2 \cdot \dfrac{b^4}{a^2 + b^2}.$ This is the same as for the sphere when $a = b$ and b is the radius of the sphere.

Experiments were described which verify the results of the front and back resistances being nearly equal, and show the effect of different forms of bodies moving round an axis in water.

March 21, 1870.

The President (Professor CAYLEY) in the Chair.

New Fellows elected :

R. PENDLEBURY, B.A., *St John's.*
E. F. EDWARDES, B.A., *Trinity.*

The Treasurer (Dr CAMPION) presented his accounts, and

congratulated the Society on the improved state of their finances.

The thanks of the Society, proposed by Professor BABINGTON, and seconded by Professor SELWYN, were cordially accorded to him.

Communications made to the Society :

(1) *On Carmine and the colouring principles of Cochineal. By* Mr RÖHRS, *Jesus College.*

In most treatises on Chemistry it is stated that carmine is a compound of a peculiar acid styled carminic acid, and animal matter, to which for the purpose of giving body to the pigment alumina or oxide of tin are occasionally added.

Numerous receipts for its manufacture have been published ; in an early edition of " Ure's Dictionary of the Arts " there are several ; and it was from one of these that I hit upon the clue to the correct theory and practice of carmine-making. In all these receipts rain or distilled water, or *river water* are recommended to be used. With rain or distilled water I invariably found failure to attend my experiments, and with river water also, unless under peculiar circumstances. These circumstances were, the presence of a notable quantity of lime in the water, existing I believe in the form of carbonate of lime dissolved in carbonic acid. This led me after numerous experiments to devise the following method of making carmine (on the small way and for the use of artists and amateurs, though I see no reason why it might not be carried out on a larger scale) which yields a pigment of exquisitely pure scarlet tint and of a richness and brightness beyond any to be now met with in the shops.

The first condition to be observed, is that the cochineal should be of the right sort. It should be by preference a Mexican black, or silver grey, hard and shelly, of rather a small starved shrivelled grain ; not pasty when ground. The colour of the ground cochineal ought to incline to foxy red, and not to purple.

Honduras black also yields a good carmine, but I prefer the Mexican cochineals.

Take 840 grains of the best cochineal : grind it well in a porcelain, not metal, mortar to a powder of the fineness of ground coffee or snuff. Intimately mix with it by grinding 40 grains of whitening, *i.e.* washed chalk or carbonate of lime. Set a 5 quart tinned iron saucepan, size No. 8, filled to about an inch of the top with rain water, on the fire to boil. This will contain about 4 to 4½ quarts. Before boiling, throw in 8 grains of bicarbonate of ammonia (carbonate of ammonia of the shops exposed for a few days to the air), and 5 grains of oxalate of ammonia (to precipitate any lime that the rain-water may have taken up from the roof of the house); when the water boils remove the saucepan from the fire, add about half an ounce of cold rain-water to throw the " water off the boil," then stir in the mixture of cochineal and whitening. Replace the lid, and leave the saucepan for about 8 or 9 minutes near the fire on the fender, so as to keep the temperature as nearly constant as possible, and just a little under the boiling point. At the end of the 8 or 9 minutes sprinkle in slowly, little by little at a time, stirring all the while with a clean wooden rod, from 37 to 40 grains of finely powdered chemically pure alum. Then transfer the contents of the saucepan to a conical tin vessel, provided with a handle and lip ; a milk-can will answer very well. This vessel ought previously to have been warmed with scalding water. Cover the can with a wooden or pasteboard cover, or the lid of a cigar box, &c., muffle the whole up in a cloth to keep in the heat, and leave it just outside the fender in front of the fire for about from 15 to 20 minutes. By this time, to quote from Ure, the " bath will be as clear as though it had been filtered," and of a bright scarlet colour. Decant cautiously about ⅔rds of the contents into a clean tinned iron saucepan, add about half an ounce fluid measure of a solution of the white of one egg in 16 ounces of water (the solution must previously have been strained through

a muslin sieve), so that about $\frac{1}{30}$th of the white of egg will have been added (or even less may be tried at first), this must be well stirred in. Set the saucepan on the fire to warm, not boil, interposing the tongs between the heated coals and the bottom of the saucepan. As soon as the saucepan begins to sing, it must be removed and examined, if the liquor be not yet curdled the saucepan must be replaced on the fire, but if curdled, the contents must be stirred and transferred to a clean tinned vessel of the milk-can form, set in a basin of cold water, to cool, and left for about twenty minutes. At the end of this time the carmine will have gone to the bottom, and the supernatant liquor may be decanted off, and kept for purposes presently to be described. The carmine will now be collected on a filter, washed with a little pure rain-water, scraped off with a silver spoon, and set on a cushion of filtering paper to dry in a dark closet. The greater part of the remaining one-third of the scarlet liquor may be decanted cautiously off the dregs and by means of two or three decantings freed from uncombined chalk and animal matter. It will yield a carmine of good quality but not so good as the first two-thirds. The carmine obtained from the first two-thirds will be of a magnificent geranium scarlet, lighter and brighter with 37 of alum than with 40, but perhaps richer with 40. I do not know where carmine so brightly scarlet can now be met with, though the best carmine made many years ago by the then well-known chemical and colour manufacturers the Bergers, and which, notwithstanding its high price, enhanced no doubt by a protective duty, was found so superior to any French carmine, as to be largely used by the artificial flower makers at Paris, was to say the least as bright as what I have made ; but there is no longer such good cochineal to be met with, as was the case then. However, I do not think that carmine is to be purchased now so bright as that made by my own method, which though based on the French method described in Dr Ure's dictionary is essentially different in its

details, especially in the precautions taken to avoid overheating the cochineal, and above all in the addition of whitening, without which it would fail entirely.

I will now detail a few experiments which I think will throw a good deal of light on the theory of carmine making; and establish the fact of the existence of at least *two* distinct colouring principles in cochineal.

(1) If no whitening be added to the cochineal, nothing but a small quantity of a dirty purplish, precipitate is obtained with *rain*-water. If an insufficient quantity such as 8 or 10 grains only instead of 40, carmine will be produced, but of a bad quality, and of a dull crimsonish colour without lustre. If 25 to 30, carmine as good or nearly so as with 40, the excess above what is necessary to decompose the alum and combine with the colouring principle going to the bottom with the dregs after the alum has been added.

(2) If carbonate of barytas or carbonate of magnesia be substituted for the whitening, dirty coloured purplish precipitates are the result.

(3) If a small quantity of ordinary carmine be burnt in a silver spoon over the flame of a spirit lamp, and the ashes digested in dilute nitric acid, filtered, and to the filtrate oxalate of ammonia and ammonia sufficient to neutralize the liquor be added, a copious precipitate falls, consisting of oxalate of lime and alumina, of which the alumina is taken up again on the addition of acetic acid.

(4) If carmine be dissolved in liquor ammoniæ diluted with about 3 times its weight of water, and the liquor filtered, a residue consisting apparently of lime and alumina remains.

(5) If the filtrate be rendered strongly acid by acetic acid, a copious precipitate ensues of what is known by the name of " precipitated carmine."

(6) If to the filtrate from which the precipitated carmine was separated, oxalate of ammonia be added, a precipitate of a

considerable quantity of oxalate of lime results, and if this filtrate be rendered neutral by ammonia and acetate of lead be added, a precipitate of a beautiful *bluish purple* ensues.

(7) If the precipitated carmine be collected, washed with a little cold water, (it is soluble in excess of boiling water) and again dissolved in liquor ammoniæ, and to the solution acetate of lead be added in sufficient quantity, a precipitate of a *reddish* purple or purplish red is obtained.

(8) This precipitate may now be carefully washed and mixed with water; the quantity obtained from about 100 grains of ordinary carmine may be mixed with about 4 ounces of rain-water, and decomposed by sulphuretted hydrogen gas. This operation requires great care, as the peculiar colouring principle or acid, and which I shall call coccineo-carminic acid, and which is here combined with the oxide of lead, is easily decomposed, and partly converted into another acid, the *purpureo-carminic* acid, the same as that combined with the lead in (6).

It is best to operate with a small apparatus composed of two six ounce bottles, containing one the sulphide of iron, and the other the creamy mixture of the coccineo carminate or, for short, c. carminate of lead. A tube $\frac{1}{4}$ inch thick externally is wide enough for the tube through which the gas passes, the bubbles of gas should not come through faster than about 3 in a second, and the operation should be arrested at the end of about 10 minutes or a quarter of an hour at most; any way, the operation must be arrested before the whole of the lead is saturated, and some of the c. carminate of lead must be reserved apart to mix with that in the bottle at the end of the operation to absorb any sulphuretted hydrogen that may be dissolved in the liquor.

I fancied that the operation succeeded better when the air was excluded as much as possible from the bottle containing the c. carminate of lead, by means of a little blotting paper loosely stuffed in at the neck.

When the operation is finished, and after the contents of the

bottle have been well shaken up with a little fresh c. carminate of lead, they must be boiled in a porcelain evaporating dish and filtered boiling hot through a double filter of fine filtering paper, and the filter washed once or twice more with boiling water, and the matter on the filter collected and boiled over again and filtered again and washed with boiling water, as the c. carminic acid is but sparingly soluble even in hot water, and adheres obstinately to the sulphuret of lead.

(9) The filtrates may then be mixed together, and the liquor rendered strongly acid by acetic acid, this will cause after some time, or immediately if the liquor be warmed, a precipitate of pure c. carminic acid, combined perhaps with a trace of gelatinous matter, this collected and dried in the shade resembles closely precipitated carmine, which indeed consists almost entirely of c. carminic acid, with the addition of a little alumina.

(10) If now to the liquor from which carmine has been extracted in the process of carmine making, a few drops of a solution of acetate of barytas be added to precipitate any sulphuric acid that may be present derived from the alum, and about an ounce of common acetic acid to precipitate any c. carminic acid that may still remain uncombined with the carmine, and the liquor be filtered, and the filtrate rendered neutral by ammonia, and acetate of lead in sufficient quantity be added, (about 150 grains for the 840 of cochineal is ample), a beautiful bluish purple or purplish blue precipitate of purpureo carminate or (for short) p. carminate of lead ensues, this must be collected on a filter and well washed.

(11) If now this precipitate (which seems to be as rich and beautiful a pigment in its way as carmine itself) be treated as the c. carminate of lead was, with sulphuretted hydrogen only, the gas may with advantage be passed through it for an hour or more, some fresh p. carminate being kept apart to absorb any sulphuretted hydrogen in the bottle at the end of the operation, and the liquor be filtered, washed with a little water, and the

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concentrated filtrate dried in a water bath, a quantity of a crystalline mass of p. carminic acid is obtained. The p. carminic collects like a dense brownish purple glaze on the evaporating dish of the water-bath, and on being scraped off with a sharp knife, assumes the form of a crystalline powder, full of shining scaly crystals. It is very soluble, and the colour of the solution a purplish red, or yellowish red, if all the sulphuretted hydrogen has not been absorbed and any part of it left to be transformed into sulphurous and then sulphuric acid.

If to the solution of p. carminic acid a little ammonia be added, the liquor assumes a rich lake or wine colour very different from the deep crimson of the solution of c. carminic acid in ammonia.

(12) If to the solution of common carmine in ammonia diffused through sufficient water, freshly precipitated gelatinous alumina be added, a beautiful crimson lake of the colour of a crimson rose or boiled red beet-root is obtained ; but if to the solution of p. carminic acid in ammonia, or to the carmine liquor neutralised after all the carmine has been got out of it, gelatinous alumina be added, a lake-coloured lake, paler, purpler, and weaker and altogether of another tint, is the result.

(13) If the liquor in (9) from which the c. carminic acid was precipitated by acetic acid be neutralised, and then acetate of lead be added, a copious precipitate of the p. carminate of lead follows.

(14) The p. carminate of lead is decomposed by phosphoric acid, and p. carminic acid results, but from the few experiments that I made, I inferred that not the *whole* of the p. carminate was thus acted on, only a part, and a reddish precipitate of a mixture probably of phosphate of lead and a double phosphate and p. carminate of lead remained.

(15) Sulphuric acid seemed entirely to decompose the c. carminate of lead, and the c. carminic acid it contained as well, a variable quantity of p. carminic being formed at the same

time; but as the results of the few experiments which I made with phosphoric and sulphuric acid on the p. carminate and c. carminate were so unsatisfactory, I did not continue them, and did not very carefully examine the products to which they gave rise.

From these experiments I infer, first that two distinct colouring matters exist in cochineal, the c. carminic acid and the p. carminic acid. That the first is extremely unstable and easily decomposed by heat and strong acids and even ammonia, the result of the decomposition being in part the second acid. For I succeeded in obtaining p. carminate of lead of the bluest tint most free from any c. carminate by boiling, for an hour or two, cochineal in water, adding acetic acid to the decoction and filtering, neutralising with ammonia, and adding acetate of lead; the boiling, as much as the addition of the acetic acid, contributing to the elimination of the c. carminic acid, by decomposition. The p. carminic acid is soluble in acids, but the c. carminic acid is nearly if not quite insoluble in dilute acetic acid even at a boiling temperature. The addition of chalk to the cochineal may act partially by decomposing the alum, and the sulphate of lime so formed may prevent the fatty acids in the cochineal from being dissolved, and soiling the carmine; but as for this purpose carbonate of magnesia would answer as well, and as it did *not*, and as lime was certainly contained in the carmine I examined, carmine I bought, as well as in my own, I infer that it is an essential ingredient of that colour.

Hence we see too why owing to the extreme instability of c. carminic acid, and to the fact that heat decomposes it, it is so necessary to avoid long-continued boiling in the preparation of a bright carmine, and why it is prudent to use so little alkali, and that saturated with carbonic acid in the operation. The excess of carbonate of lime by rendering the liquor neutral no doubt tends also to prevent the c. carminic acid from being precipitated to the bottom among the dregs, and allows of the slow

formation of the c. carminate of lime and alumina which, I take it, is the most essential part of carmine, if not all that carmine consists of; but if that were the only use of the lime, carbonate of magnesia or of barytas would answer as well, and as I have just observed, they do not. As to the salts of tin, the chlorides throw down crimson and reddish *lakes,* drying up into hard brittle substances from both the p. and c. carminates of ammonia. The c. carminic acid is probably decomposed by bichloride of tin. Actinic influences have nothing to do with the success of the operation; I have made as good carmine in dull days as in bright ones, nor, if the vessels are clean, as they should be, (for that is of primary importance, the slightest rust or impurity being evidenced by a degradation in the tint of the product,) is failure possible. The notion that fine weather is favourable to carmine making, may arise from the fact, that in fine dry weather, river water holds more lime in solution than when diluted by copious rains. With the exception of the carmine made years ago by the English firm of Berger, the best I believe has been made in France, and at Rouen, and the river Seine is saturated with chalk. The latest investigation into the colouring matter of cochineal is I believe that published by De la Rue. His carminic acid must (I imagine from the résumé of his results given in "Chemistry as applied to the Arts" by Dr Muspratt) be the same as what I call purpureo carminic acid.

The combining numbers of c. carminic and p. carminic acids would be best determined from their lead compounds, but for this purpose an accurate Liebig's apparatus and a good pair of scales are necessary, and my appliances are of the rudest and homeliest kind. I cannot lay claim to any great skill or experience as a practical or even theoretical chemist, but my experiments imperfect as they are, will I think be sufficient to prove the thesis I have advanced, viz. that of the existence of two colouring principles in cochineal, and of lime in carmine, and may perhaps draw the attention of abler chemists than

myself to this interesting subject. I forgot to state that with good cochineal the ⅔rds of the liquor from which the best carmine is made yields about 45 grains of carmine to 840 of cochineal; 40 grains is however the general average. Carmine is crimson viewed by *transmitted* light, and scarlet by reflected light; for water-colour painting as little gum as possible should be employed, barely enough to prevent the carmine when dry from rubbing off on the finger, otherwise its vivid colour will be much degraded; carmine and drawings coloured with carmine should be kept in a dark place.

Professor LIVEING mentioned some investigations, not noticed by the author, which were in favour of the theory of two colouring principles.

(2) *On a Roman Lanx and other Antiques found at Welney. By* Mr LEWIS, *Corpus Christi College.*

Having lately spent a few days in the fen country lying between Ely and Huntingdon, I was much surprised with the variety of interesting questions suggested on the most cursory survey of what is described in county histories as a dreary and uninteresting plain. Year by year the ploughman brings again to light huge trunks of sound but blackened oak; acorns and hazel-nuts, as of last year's growth; horns of red deer, perfect as when shed by the monarchs of the woodlands, who shall say how many centuries ago—objects these which tell of a primeval forest age which must have been succeeded by alternations of submergence and states of rank swampy vegetation, for in many parts horizontal seams of alluvial soil are found dividing deep layers of peat. The progress of agriculture having by arterial drainage made fertile for corn and grass spots formerly little visited, except for the flocks of wild fowl to be shot or the curious butterflies to be caught, we are able at last to begin our conjectures as to the mode of life

of the (may I say?) antediluvian inhabitants of this fen
country on which in later times the old Roman castle of Cam-
boritum looked down as a peninsula on a number of islets.
In order at once to consolidate and manure the upper stratum
of peat which years of drainage have reduced to less than
half its original thickness (within 40 years a subsidence from
nine to four feet in depth has been observed), the farmer every
eight or ten years spreads on the surface and ploughs in a
layer of stiff clay brought up by means of trenches three
feet wide at intervals of about 15 yards. Rarely are these
clay-pits opened without disclosing not only the vegetable and
animal traces of ages past, which I have mentioned, but also
implements of flint, bronze, and iron, which admit of close
comparison with those already classified by the laborious skill
of M. Keller, Sir John Lubbock, and other pre-historic archæo-
logists. Nor are clear evidences of Roman occupation want-
ing, not only as elsewhere in bulwarks against a common
enemy, the ocean (*e.g.* at Lynn), and in roads (*e.g.* that from
Denver through March to Peterborough), but again and again,
as an elevation of a few feet above the surrounding fen finds
us on what is still an island, speaking for itself in such
names as Ston-ey, Angles-ey, &c., we discover that never-
failing evidence of a Roman habitation, pottery, as well as
arms and domestic appliances for use and luxury. It is an
object of the last class which by the favour of the owner,
Mr A. Goodman, I have now the honour of submitting to
your criticism—a charger which, for reasons that may prove
satisfactory, I think may be considered of Roman work. It
was found in the spring of 1864, at the depth of 14 inches,
in the course of gault-ploughing a piece of old grass land,
about 200 yards from the Hundred Feet River at Welney,
once an islet in the district of Wella, which now comprises
the parishes of Upwell, Outwell, and Feltwell, in the county
of Norfolk. Various opinions have been suggested in regard

to the original intentions or use of the large disc of metal
thus brought to light: there can, however, be little doubt
that it is a specimen of the flat charger or dish used by the
Romans to hold a large joint of meat, or, as in a case men-
tioned by Horace (S. II. 4. 41), a boar entire (illustrated and
confirmed by an ancient fresco found near S. John Lateran at
Rome), and also serving occasionally for sacrificial rites (Virg.
G. II. 194, &c.). Such an appliance of the table was properly
designated a lanx, and the epithets, " panda," " cava," and
" rotunda," commonly applied to it by ancient writers are obvi-
ously most appropriate. To the kindness of Professor Liveing
I owe an analysis which shews that the metal of this lanx
is 80 per cent. tin, with 18½ lead, and a little trace of iron,
thus nearly corresponding with the argentarium of Pliny
(H. N. XXXIV. 20 and 48), and with certain oval cakes that
have been found in the bed of the Thames, near Battersea,
on which are stamped the Christian monogram, with the word
"spes," and the name, as it is believed, of Syagrius, perhaps
the same whom we hear of as secretary to the Emperor Va-
lentinian. One of these cakes weighs nearly 111 ounces. In
the term ἑανὸς κασσίτερος, as designating the material of which
Hephæstus made the greaves of Achilles (Il. XVIII. 612), we
probably find the earliest mention of a compound of this kind;
and Boeckh (Inscrr. I. 150, § 48) gives καττιτέρινα ἐνῴδια
(ἐλλόβια?) πέντε as occurring in a list of offerings of plate
and jewelry dedicated, in Ol. 95. 3 (B.C. 398), to the gods of
Athens. The pliability of such metal shows the strong pro-
priety of the words of Juvenal (V. 80)—

> "Aspice quam magno *distendat* pectore lancem
> Quæ fertur domino squilla."

As is seen also in the passage from Horace quoted above. Of
the lanx before us the diameter is 2 ft. 4¾ in., equal to 2½ feet
of Roman measure, the weight 30 lbs.—excessive, according
to our modern ideas, of the capabilities of servants; but Pliny

(*H. N.* XXXIII. 52) tells of a Spanish dispensator (one of the slaves of the Emperor Claudius) who had a lanx of 500 lbs. weight and eight more of upwards of 100 lbs. each, to make a complete service (if such be the true meaning of " Comites ejus octo octengentarum et quinquagenta librarum)," and naively adds, " Quæso quam multi eas conservi ejus inferrent aut quibus cœnantibus." Tertullian, alluding to the passage, calls such a dish " promulsis," meaning, I suppose, " promulsidarium." In the centre, on the upper surface, which is slightly dished to prevent the gravy flowing over, there is a circular compartment nearly nine inches in diameter, encircled with a very elaborate diapered pattern of peculiar type, produced apparently by means of the punch, chisel, and hammer : this compartment, of which the beautiful design is somewhat indistinct in the present condition of the surface, is surrounded by a bordure, decorated with trailing or branched work, in the outer circle of which may be discerned, in ten spaces, at equal intervals, certain letters, which my learned friend, Mr A. Way (who has materially aided me in the present investigation), thinks form the words VTERE FELIX ; but I fear that I cannot state more in support of this view than that there is certainly a V and an X, with eight intersecting arcs between them, and that in several of the intersections the letter E may be distinguished. Some of the letters appear to be deeply incised, while others are embossed in slight relief. On the reverse of the lanx there is a central circle in relief, possibly thus fashioned to give more substantial support and prevent the risk of bending or falling out of shape that might occur in so large a flat plate of metal when a heavy joint of meat was carried upon it. Although but few examples of the lanx are to be found in collections of Roman utensils, they have occasionally occurred. A large plane lanx of silver was acquired for the British Museum in the Blacas collection, and in Lysons' *Reliquiæ Britannico-Romanæ*, Vol. I., we find one figured of

similar style and composition, but inferior in size and quality of decoration, which was found near Manchester on the site of Mancunium, with two others—the three measured in diameter $14\frac{7}{8}$ in., $17\frac{3}{4}$ in. and 20 inches, or $1\frac{1}{3}$, $1\frac{1}{2}$, and nearly 2 Roman feet. I have failed to ascertain the existence of any pewter vessels of Roman work in Continental Museums. Their manufacture may have been exclusively carried on in Britain. It may well be worth while to consider if chemistry will not supply us with some agent that shall arrest the exfoliation, which has already done so much to mutilate the surface. Besides numerous oak trunks, the only other objects discovered on the same estate are the small weapon at hand, probably one of the earliest forms of dagger known in the bronze age, and the well-preserved antler before you: but the present tenant, Mr G. Daintree, having promised ere long to make a careful sounding of the whole field in which the treasure before us once more saw the light, I have little doubt but there is a rich and instructive harvest yet in store for the classical antiquary, especially as vases full of Roman coins have been found in the adjoining parish of Upwell.

Prof. C. C. BABINGTON remarked on a shield which was also on the table, calling attention to the absence of the usual tacks, which served for the attachment of leather for additional strength. He also gave instances of pewter vessels being found in the Thames. He commented on some of the other antiques on the table, amongst which were two stone weapons (polished) and a Roman statuette (bronze). He also called the attention of the finders of such things to the great importance of depositing them at once in some Museum, such as that of the Cambridge Antiquarian Society, where they would be safe, instead of being ultimately made toys for children and destroyed, as now was often the case.

Mr LEWIS made some further remarks on the localities where the various antiques had been found, calling attention

also to a bow which was said to have been found in the fens.

Mr BONNEY expressed his opinion that the inscription on the lanx was not UTERE FELIX, though he was unable to suggest any other reading. He called attention to an antler of a red deer on the table exhibiting cuts, and doubted whether the bow could have been found in the fen.

<div align="center">

May 2, 1870.

</div>

Mr TODHUNTER (*Vice-President*) in the Chair.
New fellow elected:

<div align="center">

CLEMENT HIGGINS, B.A. *Downing.*

</div>

Communications made to the Society:

(1) *On the best form for the ends of Measures à bouts.*
    By Professor MILLER, F.R.S.

After describing the various forms which had been commonly adopted, and pointing out their defects, the author stated that the best form was that of two 'knife edges,' whose edges were in planes perpendicular to each other, and were not straight lines, but arcs of circles, whose centre was the opposite end of the axis of the bar. In this case if l were the length of the bar, e the distance between the real and the assumed position of the point where the axis of the bar intersects the bounding surface, the amount of the error was $\dfrac{e^4}{4l^3}$; while in the forms commonly used it was $\dfrac{e^2}{l}$ or $\dfrac{2e^2}{l}$; either of which quantities were considerably greater than the error in the form proposed by the author.

(2) *Note on supposed Mollusc borings in the carboni-*
    ferous limestone of Derbyshire. *By* T. G. BONNEY,
    B.D.

The burrows described were in two localities, the first was in a band of limestone which crops out between two sheets of

toadstone on either side of Salter's Lane above Matlock Bridge ; at a height of more than 400 feet above the river Derwent. Here the burrows occur in great abundance. A specimen of these was exhibited ; and the author contended that, from the shape and arrangement of the burrows, it was impossible that they could be the work of *Pholades.* The second case described was at the bottom of Miller's Dale, in the upper part of a low cliff by the road side, about 12 feet above the present level of the river Wye. The author believed that this cliff was an artificial scarp formed in making the road. But even if the cliff were natural, he pointed out that Miller's Dale was distinctly a valley of fluviatile erosion, that there was no indication of its having been submerged in its present form beneath the sea ; and consequently that these burrows, so near the bottom of a gorge of this kind, could not be the work of any marine mollusc. He was therefore convinced that the *Pholas* theory was untenable; and believed that the burrows were excavated by *Helices.*

May 16, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communications made to the Society :

(1) *Helmholtz and Tyndall on the Theory of Musical Consonance. By* Mr SEDLEY TAYLOR, M.A.

[*Abstract.*]

The *quality* (' timbre ') of musical sounds in general has been shewn by Helmholtz to depend on the relative intensity in which the partial tones, of which they consist, are present. Dissonance depends on the occurrence of *beats* between adjacent sounds. When two fundamental tones are so related to each other that beats are not produced between any well-developed pairs of the corresponding over-tones, the interval between the fundamental tones is *consonant.*

In the case of *simple* tones, *i.e.* of such as possess no over-tones whatever, the difference between consonance and disso-nance depends on the presence of a class of sounds discovered by Sorge in 1740, and generally since known as Tartini's tones, but called by Helmholtz, who has considerably increased the series, *combination-tones*. In the case of two simple tones the interval is consonant if no audible beats are produced by combi-nation-tones with the primaries or with each other—dissonant, if otherwise.

Professor Tyndall, in the last of his published Lectures on Sound, has given a theory of consonance which differs radically from that of Helmholtz, and is irreconcilably at variance with experiment. The fundamental error of his reasoning consists in the neglect of the most essential condition for the production of audible beats between two simple tones, namely, that they must *lie near each other in the musical scale.*

(See for the proof of this in detail a Letter by the writer of this paper in ' Nature' for March 3, 1870.)

Professor CHALLIS made some remarks vindicating Dr Smith and Young from a statement made by Prof. Tyndall, with regard to the theory of consonance not being understood before the time of Helmholtz, and expressed a general concurrence in Mr Taylor's paper, so far as he had followed up the subject.

Mr TROTTER said that Helmholtz had first assigned to combi-nation-tones their true origin. When two loud tones coexist the excursions of the molecules are so large that terms of the second order arising from the combination of the vibrations become sensible.

Mr PALEY remarked upon the beats heard when a church bell has been struck, and the tone is dying away; thinking that this might result from the tin and copper not being well amalgamated in the metal.

Mr TAYLOR, followed by Mr TROTTER, said that these beats were produced by the bell not being in perfect tune throughout,

and Professor STOKES gave the same explanation, pointing out that a bell was rarely a mathematical figure of revolution.

Mr POTTER attributed these beats to the occurrence of nodal lines on bodies when sounding, which gave rise to interferences.

(2) *On a case of Asymmetry in the Human Body.* *By* Professor HUMPHRY, F.R.S.

The subject of this paper was a female in Addenbrooke's Hospital, who was born with one side of the body on a larger scale than the other, the want of symmetry being complete throughout. For example, it amounted to $2\frac{1}{2}$ inches in length of arm, and had been carried on in the right mammary gland and right side of the face, and even in the tonsil and teeth, the teeth being in a plane a little lower than on the left— the right arm and right leg stronger than left in either case; the person was in good health and well made, with not the slightest sign of paralysis. Prof. Humphry quoted a case mentioned by Broca, of a boy, aged 11, in whom asymmetry was very marked, so much so as to look as if he were made of halves of different persons put together. He also exhibited two models of a brain from Van der Kolk's museum at Utrecht, where there was marked want of symmetry. In this case there had been paralysis of the *opposite* side of the body; here it would result from deficiency of growth. It would be interesting to know whether the right or left side of the brain in the present case were larger.

A conversation took place concerning the symmetry of crystals, in which Professors MILLER, HUMPHRY, and CAYLEY joined. Mr SEELEY asked whether temporary paralysis of the mother would account for the asymmetry exhibited by the subject of the paper; Dr HUMPHRY doubted whether it would do so.

May 30, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

New Fellow elected:

F. S. BARFF, M.A. *Christ's College.*

Communications made to the Society:

(1) *On the Invention of the Camera Lucida by Wollaston. By* Professor MILLER, F.R.S.

Among a collection of scientific instruments placed under the care of the University by Mr Elphinstone, Professor MILLER had found two glass prisms cemented together and a four-sided prism of glass, together with other combinations of lenses and prisms; to some of which reference was made by Wollaston in two or three papers in the Philosophical Transactions. Professor Miller showed how these instruments might be used in discovering and approaching to the regular form of the camera lucida.

(2) *On the Frontal Bone in the Ornithosauria; with additional evidence of the structure of the hand in Pterodactyles from the Cambridge Upper Green Sand. By* Mr H. SEELEY, F.G.S.

The portions of the head hitherto obtained from the Cambridge Upper Green Sand are the back part and front part of the mouth. The specimen exhibited was part of the frontal bone, which showed that the Pterodactyle approached closely to the avian type. Mr SEELEY had for some time supposed that the fingers of the Pterodactyle had been misplaced, and the descriptions usually given and the specimens exhibited, in his opinion, fully confirmed that suspicion.

(3) *Note on a new species of Plesiosaurus from the Portland Limestone.* By Mr H. SEELEY, F.G.S.

Some time back a specimen of the fore limb of a Plesiosaur was obtained from the Portland limestone for the British Museum. Remains had also been found which had been referred to Pliosaurus. The specimen of two vertebræ which was exhibited was undoubtedly a Plesiosaur, but appeared to Mr Seeley to show certain approaches to the Pliosaurian type.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

15

October 31, 1870.

Professor Cayley (President) in the Chair.

The following Officers were elected:

President.

Professor Cayley.

Vice-Presidents.

Professor Selwyn.

Mr Todhunter.

Professor Babington.

Treasurer.

Dr Campion.

Secretaries.

Mr Bonney.

Mr J. W. Clark.

Mr Coutts-Trotter.

New Members of the Council.

Professor Liveing.

Mr Paley.

Mr Danby.

The Vitality of Paganism, an Exposition of the Doctrines of the Nuseiríyeh, a Secret Sect in Modern Syria. By E. H. Palmer, M.A., *St John's College.*

[*Abstract.*]

The paper sets out with the theory that all unrevealed religion originated in the worship of light or space, i. e. the Sky, as the

only symbol by which the conception of an omnipresent and all-pervading deity could be arrived at. The worship of the Sun and Moon are only successive developments of the same idea and symbols of what was at first itself a symbol. The five planets are next deified, and in these eight we have the germ of all mythological systems. The Sabæan religion was actually Paganism in this stage. The worship of the Sun as a corruption of the earlier worship of the Sky is hinted at in the Greek myth of Zeus supplanting *Kronos the son of Ouranos* as King of Heaven. The origin of the ancient mysteries was the preservation of the esoteric doctrine of Paganism; an attempt to recall to people's minds the fact that their deities but represent the heavenly bodies, which again are only the symbols of light; and that the latter is but an expression for the conception of an all-pervading God. Such secret teaching has always existed in the East; and as the mystic sects of Syria and Persia at the present day inherit the traditional explanation of pagan doctrines, we may reasonably expect to find pagan rites also preserved; and a knowledge of their tenets will therefore help us to understand the principles and practice of Ancient Paganism.

As the *form* of symbolism which is exoteric cannot affect the esoteric doctrines, we find that secret pagan sects adopt the outward form of the prevailing religion of the time; thus Gnosticism was clothed in a semi-Christian garb, and similarly the Sufis, Druzes and Nuseiríyeh make use of Mohammedanism as a cloak for their no less pagan doctrines.

The Nuseiríyeh worship a mystic triad consisting of Alí ibn Abi Táleb, Mohammed and Selmán el Farsi: the first is called the Meaning, the second the Chamberlain, and the third the Door.

From these proceed five other beings called Monads, to whom names, also borrowed from the companions and supporters of Ali, are given. These exercise the functions of creation and order.

The Devil is also a triune being, the three immediate opponents of Ali being taken as representatives of his avatars.

Their esoteric doctrine teaches that these personages only typify the heavenly bodies : thus Ali is the sky, Mohammed the sun, and Selman el Farsi the moon, while the five monads represent the planets known to the ancients, and their functions exactly correspond with those of the heathen gods whose names the planets bear.

The Nuseiríyeh believe in the transmigration of souls; they were originally stars, but fell through disbelief; those therefore who act well in this life will be restored to their celestial rank, while the bad will pass through successive stages of degradation in a future life.

Amongst other curious observances, they commence their prayers by the distribution of branches of olive or fragrant herbs to all the congregation, who on the repetition of a certain prayer place them solemnly to their noses. This is undoubtedly the rite mentioned by Ezekiel as appertaining to Syrian sun-worship : "Is it a light thing that they commit the abominations which they commit here ? For they have filled the land with violence and have returned to provoke me to anger ; and, *lo, they put the branch to their nose.*" Ezek. viii. **17.**

They also make use of libations of wine, considering this as a symbol of the sun from its brightness and revivifying effects. Our own social practice of "passing the bottle the way the sun goes round," is a relic of the same Pagan superstition.

The Nuseiríyeh number about 5000 in Syria, and inhabit principally the districts around Laodicea and the mountains north of Aleppo.

The author then gave a slight sketch of the Sabæan creed, and concluded by repeating his belief that the principles of what may be termed astronomical worship lie at the root of all Pagan systems; and that in the tenets and philosophies of these Eastern sects will be found much that will assist in the

interpretation of the ancient mythologies and throw considerable light upon the nature of the Greek and Roman rituals.

Mr GEORGE WILLIAMS enquired whether Mr Palmer could give any information about the Metawaleh, and whether their tenets in any way resembled those of the Nuseiríyeh.

Mr PALMER replied that the Metawaleh were simply a sect of the Sheans, but that there was a slight connexion in that they held Ali in peculiar respect.

Dr CAMPION enquired whether the Nuseiríyeh practised sacrificial rites.

Mr PALMER replied that they sacrificed sheep and offered libations of wine ; and that not later than 300 years ago (on the authority of Arab historians) they offered human sacrifices.

Nov. 14, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communications made to the Society :

(1) *On a pipe in the chalk at Alum Bay.   By* Professor LIVEING.

The peculiarity of this pipe is that it has a horizontal or nearly horizontal direction. It is seen at the S.E. corner of Alum Bay, where the surface of the chalk has been exposed by the washing away of the abutting clays, and presents no uncommon features except its direction ; it is lined pretty equally all round with a layer of weathered chalk having a banded appearance from infiltration of iron, and is filled in the usual way with loose material, including some large flints such as are strewn over the surface of the chalk at its junction with the clays above. It is hardly possible to resist the conclusion that this pipe was formed before the chalk assumed its present highly-inclined position, for if it were merely a bent arm proceeding

from a pipe passing downwards from the present surface it must have left its trace above. If this be so, it will follow that the chalk of the Isle of Wight must have been sub-aerial at a time previous to the elevation of the Weald area; at least, if the theory of the formation of pipes by the percolation of water be accepted.

Professor Liveing made some remarks also on the shattered state of the flints at the same locality. As the loose flints on the eroded surface of the chalk which are imbedded in the superincumbent clay are shattered equally with those which retain their original position in the chalk, he inferred that it was probable that the shattering was not due to the great movement by which the chalk was placed on end, since although it is conceivable that the shock of such a movement communicated as a vibration through an unyielding mass like chalk may have shattered the flints which opposed a resistance to such a vibration, it is difficult to suppose that so yielding a matrix as clay could forcibly impress on flints such a vibration as to crack them through and through in every direction. He thought that either some other cause of the shattering must be sought, or that it must have taken place at some anterior period before the flints were weathered out of the chalk, and therefore before the deposition of the clays above.

(2) *On phenomena connected with denudation observed in the so-called Coprolite Pits near Haslingfield.* By Mr O. FISHER.

This paper contained an extension to the neighbourhood of Cambridge of observations heretofore made by the author, and described in former papers in the Journal of the Geological Society and in the *Geological Magazine.* They relate to the condition of the upper portion of the sections as seen as well in the coprolite pits as elsewhere. The upper three or four feet consist of travelled material, which in the cases described

consisted of portions of the coprolite bed transported in an unscattered condition from their original positions, shewing that some agency must have acted upon them to push them laterally over the surface, different from that of running water, whether in the form of rivers or of rain. And since it is evident that the same agent which has moved the superficial beds must be that which was engaged in the work of denudation, it was argued that rain and rivers have not been the sole influences to which the configuration of the landscape is due.

The previously published papers of the author on this subject point to land-ice as the denuding agent, and give reasons for supposing that the climate may have been sufficiently rigorous for such a condition about 100,000 years ago.

Mr BONNEY expressed an opinion that the principal features of the district might more easily be accounted for by the action of lateral streams, aided by rain, at a period when the rainfall was greater than at present, and that the contortions and disturbances of the bed would better be explained by the grounding of small bergs floated off from an ice-foot than by the pressure of a glacier passing down the valley. He also thought it improbable that the valley had been occupied by a glacier since the Boulder clay period.

Mr FISHER replied that he could not interpret the phenomena as indicative of other than the action of an ice-sheet such as now enveloped parts of Greenland.

With reference to Professor Liveing's communication, Mr Fisher thought that the formation of the pipe at Alum Bay might be referred to the period of the Thanet Sands, which are unrepresented in the Isle of Wight, when denudation would naturally be taking place in that locality.

November 28, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communications to the Society:

(1) *On a model of an electro-motive machine.*

(2) *On a model for transferring rotatory motion to a distance by means of a single wire.*

(3) *On a method of describing ellipses and* (4) *of drawing in perspective.*

(5) *On a steam-ship for conveying trains from Dover to Calais.* *By* J. C. W. ELLIS, M.A. *Sidney.*

The following are abstracts of Mr Ellis's explanations:

i. Electro-motive machine.

Consisting of three springs acted upon by electro-magnets successively. The springs, or elastic levers, are easily bent by the at first feeble magnetic influence, and as they approach the magnets resist with greater force but are overcome by the increased power of the magnets, and so the effect of the magnets is equalized and prolonged.

ii. Rotation conveyed to a distance by means of a single wire.

The original motion may be either one of rotation or a rectilinear backwards and forwards motion. Upright posts revolving about hinges at the foot carry the wire, which may be very fine and light by using leverage. The wire may be turned through any angle by applying a stay-wire to the post. The wire is finally applied to a wheel carrying a rachet, which drives a heavy fly-wheel. The rachet-wheel is drawn back by a spring. The advantage of this method consists simply in the ease with which it may be put up, its cheapness, and from its requiring no alteration or oiling. It is suitable for conveying power from a water-wheel to a farm for churning, gorse and chaff cutting, &c.

iii. A method of drawing ellipses.

A square frame hinged at the corners has a number of parallel strings, on these are arranged a number of concentric circles marked on the strings by beads or paint. When the square is drawn into the form of an oblong the circles become ellipses, and the two elastic strings stretched from corner to corner are the diameters, the equi-conjugate axes being parallel to the sides of the frame. This is manifest from the equation $x^2 + y^2 = a^2$ in the square remaining as $x^2 + y^2 = a^2$ in the rhombus. If ϕ be the angle of the rhombus, $\sec \frac{\phi}{2} = e$ gives the eccentricity. Or the portions of the elastic strings forming the diameters might be measured to suit the particular ellipse required. The frame can then be placed on a piece of paper and the ellipse dotted off between the parallel threads. Properties of ellipses and other curves may thus be compared with similar properties referred to rectangular co-ordinates.

iv. Method of drawing in perspective.

$ABCD$ is a drawing frame.

The base CD is bisected in E. EFG is perpendicular to CD on the plane of the frame and projecting below the frame. EG is the distance of the eye. GF the height of the eye. If the plan of any building be drawn to scale at its true angle to and distance from CD, any point of it, K, is thrown into perspective thus : an elastic thread extends from F to G; it is stretched to K; let GK meet CD in M, and let MS perpendicular to CD meet KF in S; S is the perspective of K. The corresponding height of the KT is found by producing MS to meet TF in Z. This operation is facilitated by drawing on paper ruled parallel to a side of the frame perpendicular to CD.

v. Steam-ship to convey trains from Dover to Calais.

Two tubes of wood (or iron) 200 metres long and 4 metres diameter give carrying power of tubes when completely submerged = 5000 tons. Two parallel lines of rails to take trains,

and closed by water-tight sliding-doors. Engines, central pad-
dle-wheel, refreshment rooms, &c. between lines of rail. Dia-
meter of wheel 14 metres. Height of deck 6 metres. Weight
of vessel 1200 tons; weight of engines (of 500-horse-power)
+ weight of coal and stores 200, + weight of train of twenty
carriages 200 tons. Total, 2100 tons. Advantages—great
speed, no oscillation. Tubes in compartments, and supplied
with pumping apparatus connected with engine.

The PRESIDENT indicated a simple method of drawing an
ellipse which he was in the habit of using himself.

On the Aurora Borealis. By JAMES STUART, M.A. Trinity.

There are several different kinds of Auroras, arches, bands,
converging lines, general luminousness of the sky, &c. In some
instances the convergence of the bands is due to perspective.
The year of maximum aurora occurs every ten years, and at the
same time as that of maximum sun spots, and of maximum per-
turbations of magnetism. Auroras in the northern and southern
hemispheres are frequently simultaneous. Before an Aurora in
the northern hemisphere and during the first part of the dis-
play the magnetic needle is deflected to the west; during the
Aurora it makes frequent and violent excursions and then is
deflected toward the east. In the southern hemisphere it is
deflected also but in exactly the opposite way. This may be
accounted for by currents running in the earth from the Poles
to the Equator. De la Rue supposed these to be caused by
electricity conveyed upwards at the Equator by evaporation,
and thence to the Poles by the winds in the upper air, whence
it was discharged into the earth at the Poles. The difficulty of
such a thing is the nonconducting nature of the air. If a cloud
of some interplanetary dust or medium of some kind were to
come near the earth, the lower parts of this getting mingled with

the earth's atmosphere would acquire a velocity of rotation with the earth; electric currents would thus be generated owing to different parts of this medium cutting the earth's magnetic lines of force at different angular velocities; and the currents thus generated would be of the direction required to account for the perturbations of the magnetic needle. Such currents would also produce earth currents. The notion of meteoric haze being that which causes the Aurora may be connected with the fact that of 19 displays of Aurora selected from upwards of 250 during 13 years as being very brilliant, five occur on or about the 17th of November; and on ten of the 13 years there was an Auroral display at that time. The spectrum of the Aurora coincides with that of the zodiacal light and with that observed in a sky filled with luminous haze. Of Auroras observed at Dunse from 1840 to 1850 seventy-five were connected with stratification of cirrus clouds, the stratification of such clouds being parallel to the magnetic meridian while they moved slowly from S.W. to N.E. In those Auroras which have a "corona" it is in the prolongation of the direction of the magnetic dip. During an Auroral display the line of the dip seems generally to become more vertical.

Professor CHALLIS corroborated the account of the cloud coming and going, which on one occasion had been seen by himself at Cambridge, and Sir John Herschel at Collingwood. He had calculated the height of an Auroral arch at 175 miles. The streamers, he believes, go up hundreds of miles. The other phenomena mentioned by Mr Stuart about 175. The apparent convergence of the streamers is only an effect of perspective. He considered the streamers magnetic, and the Aurora produced by transverse streams. That there were two kinds of Auroras, one local, the other due to extraneous action. The Astronomer Royal had found that only the latter corresponded with disturbance of the needle. Auroras are said to be very common at Behring's Straits.

Mr POTTER said he remembered some brilliant displays not in a ten-year period, especially one of 1833; and he made some remarks in corroboration of Professor Challis' statements, and objected to the " dust " theory.

Mr STUART mentioned that the spectrum of the Aurora had been observed and shewn the same line between D and E, as had the zodiacal light; he briefly replied to Mr Potter.

Professor SELWYN exhibited some heliographs of the sun, extending over a period of 12 days, ending Sept. 30, all which had been very favourable for photography, at the time of the sun's maximum. They shewed a very remarkable number of spots. Another series, taken from Aug. 22—29, shewed also a large outbreak of spots. He called attention to the fact that in the northern and southern hemispheres, tornadoes went as the deflections in the magnetic needles, mentioned by Mr Stuart, viz. in opposite directions; and so did the sun's spots.

February 13, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communication to the Society:

On the Operations of the Great Trigonometrical Survey of India in connexion with Geodesy. By Col. J. T. WALKER, R.E., F.R.S., *Superintendent of the Survey.*

[*Abstract.*]

After pointing out that there are three stages in the operations of a scientific national survey, namely, the trigonometrical or geodetic basis, the topographical delineation of the ground, and the construction of the maps, Col. Walker observed that the first stage has frequently been ignored, and that during the last century this was the case in India, surveys being carried

on without any basis of operation, and afterwards compiled into a general map in which the positions of a few of the chief towns in each province were determined by astronomical observations. Such determinations are always of questionable accuracy as the basis of an exact survey, even in the present state of science ; and a century ago the tables of the places of the heavenly bodies were far less accurate than they are now, and frequently caused gross errors of position. Towards the close of the last century Major Lambton projected a "Mathematical and Geographical Survey" of southern India to determine by triangulation from measured base lines the positions of a number of permanent geographical marks to be afterwards the basis of a general survey of the Peninsula ; but he pointed out that before the latitudes and longitudes of these marks could be correctly computed from the data furnished by the triangulation, the figure of the earth should be known with accuracy, and he suggested that his operations should be carried out in such a manner as to answer the requirements of a geodetic as well as of a geographical survey. His proposals were approved of by the Government, and the operations which he carried out were the commencement of the Great Trigonometrical Survey of India.

Col. Walker shewed that the combination of objects in a triangulation which is intended to serve the purposes of a geodetic as well as of a geographical survey necessarily introduces a very high order of accuracy into the operations, and that the Indian survey has derived much advantage from this circumstance. He described the nature of the operations of the survey, the manner in which they are conducted, and the instruments which are respectively used in making the linear and the angular measurements. The probable errors of the base lines are shewn to be $= \pm 2.6$ millionth parts of the length measured, corresponding to a probable error of 108 feet in the length of the polar axis of the earth. The probable errors

of the angles measured with the great theodolites range from $\pm''\cdot 2$ to $\pm''\cdot 5$. The probable errors of the trigonometrically deduced ratios of the sides of the triangles are functions of those of the angles and of the geometric conditions of the triangulation. When the number of triangles is small the probable errors of the trigonometrical ratios are less than those of the ratios of the base lines, but in certain representative chains of triangles, averaging 575 miles in length and composed of a large number of triangles, the probable errors of the trigonometrical ratios of the base lines at their extremities are about three times those of the linear ratios. Thus the trigonometrical and the linear operations are fairly on a par with each other as regards accuracy.

Col. Walker then proceeded to give an account of the work which has been completed up to the present time, and shewed what remains to be done to finish the programme of operations. He pointed out that the present desideratum in geodesy is not so much a better determination of the mean figure of the earth, as of the variable figure at different parts of the earth's surface ; and he stated that the operations of the Indian survey will, when supplemented by appropriate astronomical operations and differential determinations of longitude by the electric telegraph, furnish a number of meridional arcs and arcs of parallel which will be of great geodetic value.

The paper closed with an exposition of the methods which have been introduced by the author for the final reduction of the whole of the triangulation, so as to render all the parts consistent and harmonious.

Mr Ellis asked if there were any signs of the earth's expansion or contraction during the measuring of a base line.

Col. Walker said it could not be detected while an operation was going on. However, after an earthquake in Eastern Cachar the officer in charge asserted that the distance of some of the stations had been altered, but it was not certain whether

this was the case. To discover an alteration there would be need to remeasure the base line after an interval of time had elapsed.

Professor CHALLIS said it had been stated that there was an abnormal deviation of the plumb-line south of the Himalayas, and asked whether this had been observed on the north also, where, according to Col. Walker, some surveys had recently been made.

Col. WALKER said the surveys on the north side were too rough to be of value for geodetic purposes. On the south side of the pendulum observation shewed a deficiency of density as the hills were approached, and an increase on proceeding southwards towards the sea.

Some conversation followed, in which Prof. Adams, Prof. Miller, and Col. Walker took part.

February 27, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communication to the Society.

On Observations made at San Antonio on the Total Solar Eclipse of 22 Dec. 1870. *By* W. H. H. HUDSON, M.A. *St John's*.

I propose to lay before the Cambridge Philosophical Society some account of the recent English expedition to observe the Total Eclipse of the Sun, which occurred on the 22nd of Dec. 1870.

This is not the place to describe the difficulties which had to be surmounted before the expedition could start at all—how some one had blundered and sent in the application to the wrong department—the natural rebuff received from a government subordinate—the unphilosophical huff thereat—the sus-

pension of all preparations—the discovery of the right department—the sudden hurried arrangements : there are those here who are better able to inform you on these points than I. But suffice it to say that, in spite of all indecision and bungling, two expeditions left these shores ; of which one went overland to Sicily—strange to say, these were shipwrecked ; and the other went by sea to Spain and Africa, having weathered what I may call the Professor's storm on the way.

The members of the latter expedition mustered on the evening of the 6th Dec. on board H.M.S. Urgent, Captain Henderson, to the number of about 35 ; there they were divided into three detachments, each under a separate leader. One under Dr Huggins went to Oran in Algeria, another under Captain Parsons went to Gibraltar.

The third party, to which I had the honour to belong, was under the guidance of Father Perry of Stonyhurst, and met with better success : we landed at Cadiz, separating there from our companions on the ship. We were thirteen in number and were distributed to our several duties thus: four were to observe with the spectroscope, four with various polariscopes, four to sketch, and one to keep the time and make general observations.

The English expedition was not the only one in the neighbourhood of Cadiz. The Americans were a very strong party at Jerez (or Xeres), and Lord Lindsay, with great public spirit, had equipped a complete party, who were stationed at La Maria Luisa, about five miles west of Jerez. The Spaniards, who had a very fine observatory at San Fernando, near Cadiz, and to whose courtesy we were greatly indebted, sent a party to San Lucar, a town at the mouth of the Guadalquiver, about ten miles north-west of Lord Lindsay's position. Our temporary observatory was at San Antonio, intermediate to Puerto de Santa Maria and Jerez, about three miles from the former and five or six miles from either Lord Lindsay or the Americans.

Description of Instrument. The instrument used was a

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refracting telescope by Dollond. The breadth of the object-glass was $3\frac{7}{8}$ inches; the focal length about 4 feet; the eye-piece was a negative one; the magnifying power 40.

There was a diaphragm in the eye-piece of the shape of a long parallelogram between the field and the eye-lenses. Outside the eye-lens was a double refracting prism of Iceland Spar, which caused such a separation of the images that when it was a maximum the short sides of the parallelogram were in the same straight line and the adjacent long sides just overlapped. This was the position in which it was used. The cap containing the diaphragm was furnished with a touch-mark consisting of a projecting spoke. When in adjustment as above described this mark was parallel to the short side of the parallelogram com-posing the diaphragm, and marked the plane of polarization of the image remote from it when the whole apparatus was turned round so that the difference of intensity was greatest.

A cardboard tube lined with black velvet was fixed at the object-end of the telescope, projecting 14 inches from the object-glass to prevent reflexion from the interior of the tube.

Experiments previous to leaving England : None.

The instrument was kindly lent by the Master and Fellows of St John's College, to whom my thanks and those of the Expedition are due. It was borrowed by the advice of Pro-fessors Stokes and Adams, the former of whom inspected it for the purpose, the latter recommended it from his previous ac-quaintance with it. It was only decided to ask permission to borrow it just in time to have cases made to convey it and its stand. It was therefore obliged to be immediately dismounted and could not be used. The eye-piece was arranged by Mr Ladd in accordance with the instructions of the Organising Committee, and was put into my hands in the railway carriage proceeding from Waterloo Station to Portsmouth on my way to join the Urgent. Consequently there were no experiments before leaving England with the instruments used.

Experiments at San Antonio.

On the Friday (the 16th) preceding the eclipse the instruments were fitted up at San Antonio, three miles or so from Puerto de Santa Maria ; in this operation Lord Lindsay kindly afforded most valuable assistance. On that and every subsequent day till that of the eclipse, Thursday the 22nd, I was employed practising the manipulations of the telescope and observing the light from the sky, the clouds, the moon and various terrestrial objects for polarization.

At first the light from almost every object seemed to be polarized : this was accounted for by the want of perfect blackness in the tube of the telescope, and supposed to arise from reflexion in the interior. To correct this a projecting nozzle lined with black paper was first tried but found insufficient. Black velvet was next had recourse to. and apparently with success.

On the night of the 17th or rather the morning of the 18th I was observing the moon and noticed that the light appeared to be polarized. Not expecting to have been able to detect this polarization of the light reflected from the moon I called out to Mr Moulton who was observing at the same time, and he had also seen bands across the moon with his instrument.

On repeating the observation I detected no polarization and Mr Moulton saw no bands. We were led to account for this by the presence of thin clouds : when the clouds were thick and when the sky was quite clear I could detect no difference of intensity on turning the analyser. When there were thin clouds, and even when no clouds were visible to the eye, but when their existence was rendered probable by the neighbourhood of visible clouds, the polarization became manifest.

I subsequently repeated these experiments near the sun at noon when the sun was so clear that I had to use a dark glass ; there were however light clouds about. I observed the sky in the immediate neighbourhood of the sun, approximately within

a radius of its limb, and at several parts I found the light polarized, the plane of polarization being always radial. Some of these observations Mr Ladd verified at the time.

I detected no polarization when thick clouds were on the sun, either on the sun or in its immediate neighbourhood; but at a greater distance, 2 or 3 radii from the limb, I found polarization, and again the plane was normal to the limb.

Observations during the Eclipse.

I repeated these observations during the progress of the eclipse after first contact and before totality. At $11^h. 8^m. 10^s.$ (11. 15. 30 G. M. T., time taken by Capt. Toynbee) just as the moon's limb was in contact with a spot (the second of two spots near together) I found polarization on the moon's limb: immediately after contact I found the same again. Later at $11^h. 32^m.$ (11. 39. 20 G. M. T.) the polarization was more decidedly marked, and I determined its plane to be at an angle of 45^0 to the horizon.

Again at 3^m before totality, when thick clouds came on, I found no polarization visible.

Observations during and about Totality.

$1^0.$ I watched for the first appearance of the corona in the East (apparently left) limb of the moon: this I saw at $12^h. 7^m. 40^s.$ (12. 15 G. M. T.).

$2^0.$ I tested the corona for polarization before totality and found none.

Note. This observation was not completed before totality; in the course of it I heard from behind me a shout of "The Corona!"

$3^0.$ I tested the moon's surface for atmospheric polarization. I found that it was visible and that its plane was the same as that I had determined before totality.

$4^0.$ In the course of this observation I noticed that the brightness of the moon was very considerably more than I expected: not so much less than before totality. I agree in

the description I had seen of its "green velvety" appearance. The green was like that of the olives that were commonly on the dinner-table in that neighbourhood, but greener and less brown. The clouds drifting across the moon were perceptible, and the mountains in the moon were not.

5°. I examined the corona for polarization near the apparent upper surface of the moon; I found that it was apparently polarized and that the diaphragm was vertical when the two images were of equal intensity. I did not determine its plane, but it must have been inclined at 45° to the horizon and therefore neither radial nor tangential.

6°. Time was nearly up. I took my eye from the telescope and for a few seconds saw the general appearance of the corona and prominences. I distinctly noticed the quadrilateral shape of the corona and that the sides of the quadrilateral were roughly horizontal, and vertical. The greatest extension was to the N.W. and was about $\frac{3}{4}$ of the moon's diameter.

7°. I returned to the telescope to watch for the final disappearance of the corona after totality. This occurred at 12^h. 10 . 30^s. (12. 17. 50. G. M. T.).

I had seen the corona for 2 . 50^s, about $\frac{2}{4}$ of a minute longer than the totality.

8°. Immediately after totality I hastily made a sketch of the impression scarcely gone from my eye of what I saw without the telescope; this was very rudely and almost automatically done, but agrees very closely with Mr Browne's sketch.

Mr Browne was standing by my side to make the sketch: he did not see mine nor I his till both were completed.

Conclusions.

I draw the following conclusions from the observations made during the eclipse and during the previous week.

1°. All observations of polarization made with telescopes that have not been previously tested for blackness inside, are utterly untrustworthy.

2⁰. That the appearances of polarization are presented when light shines through a thin cloud, whether the said cloud be visible or invisible.

3⁰. That this effect is not produced by clouds above a certain thickness. (N.B. I estimate the thickness of a cloud by its darkness : the quantity of light it absorbs.)

4⁰. I believe that the polarization of the corona which 1 detected was simply due to the intervening atmosphere.

I am not perfectly certain of the absolute success of the black velvet arrangement. I think that there may have been some slight reflexion in the tube ; had the light been stronger this might have had a perceptible effect ; as the light was so much cut off by clouds, I believe that it was practically successful.

I believe the light was quite sufficient and the instrument sufficiently sensitive to enable me to speak with great confidence when I determined that there was polarization, and to prove that when I failed to detect it it must have been very small. My determination of plane is certainly within 5⁰ and probably closer.

So much for my own observations. I now add a few words on the general results of the expeditions as far as they are yet gathered up.

1. From the spectroscope we learn that the light of the corona contains the hydrogen lines and also the unknown line 1474.

2. The shape of the corona is not circular, but roughly quadrilateral; it is broken by indentations and by a conspicuous V-shaped gap towards the S.E. corner, seen not only at three stations in Spain but also in Sicily.

3. The inner brighter corona, which is supposed to have a defined outer boundary separating it from the outer corona or glory, is probably an optical or subjective effect.

4. The question of the polarization of the corona still re-

mains open; there is not a sufficient number of accordant results free from suspicion, either on account of unfavourable sky or inaccuracy of instruments.

Professor W. G. ADAMS then exhibited photographs by the oxyhydrogen lime-light.

The photographs shewed the corona as seen from Sicily, having been taken at Syracuse by Mr Brothers. In exhibiting the photographs, Professor Adams called attention to various points of interest in them.

There were also passed round copies of the American photograph taken at Jerez in Spain, of Mr Brothers' photograph, also an enlarged copy of the latter and a reduced copy of the former, so as to bring them to the same scale, mounted so as to shew the resemblance between the two photographs taken at a distance of 1200 miles apart.

There were exhibited on the screen some maps to shew the geographical position of the observing stations, and, side by side, drawings made by Mr Hudson and by Mr Browne, of Wadham College, Oxford, from their sketches of the corona.

Mr CLIFFORD, who was with the Sicilian Expedition, described its comparative failure, but said that some observations made when the disc of the moon was comparatively free from clouds confirmed Mr Hudson's to a great extent.

Mr MOULTON, who was stationed at San Lucar, was of opinion that the polarization seen was attributable to defective instruments. Experiments had proved that polarization was not observed when the polarizer was placed, not at the eye-end, but at the other end of the instrument. The instrument had been thus used by the American observers, and they had never been able to detect radial polarization. His own experience, however, would not throw much light on the subject, owing to the tantalization of the clouds at the time of his observations, and when the eclipse was approaching totality. He could

mainly corroborate Mr Hudson; the observation of the V gap, however, was wanting.

He then read a very interesting communication from Father PERRY, summing up the results of the spectroscope observations at the various stations.

Starting outwards from the photosphere, Prof. Young and Mr Pye had proved the existence of a thin absorption band covering the photosphere, and ordinarily giving rise to the dark lines: this, as soon as the photosphere was eclipsed, burst forth as an innumerable mass of bright lines, to last only for a second or two, and then itself to be eclipsed: passing through the chromosphere, we come to an outer layer of cooler hydrogen, whose tale has been told by the spectroscope of Mr Abbay, and then at last to a hollow shell of green vapour, lighter than hydrogen, whose spectrum is the bright green line 1474, extending into space far beyond any measurable limits, and which will probably enter largely into all future theories of the ether of space.

Mr CLIFFORD added a few words, maintaining that the disturbing causes did not apply to the instruments which he was using.

<center>*March* 13, 1871.</center>

<center>The PRESIDENT (PROFESSOR CAYLEY) in the Chair.</center>

New Fellows elected:

H. M. TAYLOR, M.A.
M. R. PRYOR, B.A. } *Trinity College.*
J. W. L. GLAISHER, B.A.
H. M. GWATKIN, M.A., *St John's College.*
Rev. B. WALKER, M A., *Corpus Christi College.*

Communications made to the Society:

On the Attraction of an infinitely thin Shell bounded by two similar and similarly situated concentric ellipsoids on an external point. *By* Professor ADAMS.

No problem has more engaged the attention of mathematicians, or has received a greater variety of elegant solutions, than that of the determination of the attraction of a homogeneous ellipsoid on an external point.

Poisson's solution, which was presented to the Academy of Sciences in 1833, is founded on the decomposition of the ellipsoid into infinitely thin shells bounded by similar surfaces. By a theorem of Newton's. it is known that such a shell exerts no attraction on an internal point, and Poisson proves that its attraction on an external point is in the direction of the axis of the cone which envelopes the shell and has the attracted point for vertex, and that the intensity of the force can be expressed in a finite form, as a function of the co-ordinates of the attracted point.

In 1834, Steiner gave, in the 12th volume of Crelle's *Journal*, a very elegant geometrical proof of Poisson's theorem respecting the *direction* of the attraction of a shell on an external point. He shows that if the shell be supposed to be divided into pairs of opposite elements with respect to the point in which the axis of the enveloping cone meets the plane of contact, then the resultant of the attraction of each pair of such elements acts in the direction of the axis of the cone, and consequently the attraction of the whole shell acts in the same direction.

About three years later, M. Chasles showed that Poisson's solution might be greatly simplified by the consideration that the axis of the enveloping cone is identical with the normal to the ellipsoid which passes through the attracted point and is confocal with the exterior surface of the shell.

This mode of enunciating the direction of the attraction has the advantage of making known the level surfaces with respect to the attraction of the shell on external points.

In 1838, M. Chasles presented to the Academy of Sciences a very simple and elegant investigation, in which he arrives at Poisson's results respecting the attraction of a shell on an external point, by a purely synthetical method.

M. Chasles' method is founded on Ivory's well-known property of corresponding points on two confocal ellipsoids, and on some elementary propositions in the theory of the Potential.

Struck by the simplicity and beauty of Steiner's method of finding the *direction* of the attraction of a shell on an external point, the author of the present paper was induced to think that by means of the same method of decomposing the shell into pairs of elements employed by Steiner, a correspondingly simple mode of determining the *intensity* of the attraction might probably be found. The author has been fortunate enough to succeed in realizing this idea, and the result is the method contained in the first part of the present paper.

This method is throughout quite elementary. It requires the knowledge of only the most simple properties of ellipsoids, including Ivory's well-known property respecting corresponding points on two confocal ellipsoids.

The proof of the theorem respecting the direction of the attraction differs from that given by Steiner, and harmonizes better with the method employed for determining the intensity of the force. No use is made in this method of the properties of the Potential.

The second part of the present paper is devoted to what the author considers to be an improvement on M. Chasles' method of determining the attraction of a shell on an external point. Its novelty consists in the mode in which the *intensity* of the attraction of the shell is found. M. Chasles first compares the attractions of two confocal shells on the same external point:

He then takes the outer surface of one of these shells to pass through the attracted point, and having found the attraction of this shell by a method applicable to this particular case, he deduces from it the attraction of the general confocal shell. Now it may be remarked on this that the method of finding the attraction of the shell contiguous to the attracted point does not seem free from objection, and also that it may be doubted whether it is legitimate to include this limiting case under the general one without a special examination. If, in order to remove these objections, special considerations are introduced, the proof is thereby deprived of its simple and elementary character. Whether these criticisms on M. Chasles' method are well founded or not, the author thinks that mathematicians will not be displeased to see a direct determination of the attraction of a shell on an external point without the intervention of another shell whose outer surface passes through that point. In order to make the paper more complete, the author briefly shows how from the expression for the attraction of a shell, we may pass to the expression the integral of which gives the attraction of a homogeneous ellipsoid on an external point.

On a theory of the forms of floating leaves in certain plants. By W. P. HIERN.

Consider the curved margin of an undivided portion of a leaf which floats in a stream exposed to the resistance of the current; suppose that the power of growth is exerted equally at all points of the margin, tends to push the margin normally outwards so as to oppose rather than co-operate with the current, and is just balanced at the instant considered by the other mechanical forces which act on the margin; and further suppose that the margin remains as a flexible curve with tangential tension but not submitted to either normal strains or wrenching couples.

It then follows from merely mechanical reasons that the tangential tension is the same at all points, and that the form of the portion of the margin at the instant under consideration is determined by one of the following intrinsic equations:

$$\tan\left(\frac{s}{l} . \cos\alpha\right) = \cos\alpha . \tan\phi, \text{ or } e^{\frac{2s}{l} . \cot\beta} = \frac{\sin(\beta + \phi)}{\sin(\beta \sim \phi)},$$

according as the vigour of g owth is more or less than sufficient to overpower the direct resistance of the current.

In these equations s represents the length of the arc of the margin measured from that point of it where its tangent is in the direction of the current to the point where the tangent is inclined to that direction at the angle ϕ; and l and α or β are quantities dependent only upon the proportional values of the tangential tension, the power of growth and the direct resistance of the current. The first equation when traced furnishes a series of separate ovals (but not ellipses) the longest diameters of which all lie on one straight line perpendicular to the direction of the current; the second equation furnishes a pair of catenary-like curves with their convexities opposed to each other, which become actual catenaries when the power of growth would just balance the direct resistance of the current. Parts only of these curves are applicable to the case of the portions of the leaf-margins according to the original hypothesis, and in no case are those parts of the curves applicable which correspond to points where ϕ lies between $180°$ and $360°$.

After the leaf-margin ceases to be flexible, as for instance after the completion of its growth, the investigation can be extended to calculate the tangential tensions, the normal strains and the wrenching couples to which it is then submitted at the different points of the margin; and tolerably simple expressions are found for them.

The above equations are only suitable for those leaves in which the structure is pretty uniform in all directions, as in

plants either with very little structure or with highly reticular venation. For many monocotyledonous plants some modification is necessary in the mechanical hypothesis, and curves more elongated in the direction of the current are obtained.

Mr SEELEY asked whether there was any corresponding law to be discovered for leaves which were not floating but aerial : he thought it might be possible to refer all leaves to such laws. With regard to the influence of pressure on growth in leaves, so long as it did not override vital force in the plant, he thought it fell in with what was to be observed in animal growth, which required intermission of pressure, for continuous pressure would stop growth.

Mr HIERN said that in the case of aerial leaves the problem was far more difficult, seeing that the water neutralized gravity and the edges of the leaves were more flexible. He had considered that case a little, but preferred to discuss the simpler case first, with the hope of at some future time working out the more complex. He pointed out that there always was some little pressure on leaves from surface currents.

Dr PAGET asked whether Mr Hiern had tested his theory by some simple experiments ?

Professor CAYLEY asked whether he had examined the effect on the leaves of the same plant in still and running water ?

Mr HIERN said that the forms of leaves were apparently modified by circumstances, and that the experiments suggested would not be easy.

On the effect of exhaustion and inflation of the tympanum in deadening sounds, and on the test of loudness. By Mr MOON.

The author discussed the problem "Why, when the tympanal cavity is either exhausted or inflated, low tones are more affected than high tones?" Before doing this he considered the ordinary explanation of what is the test of the loudness of a

sound, viz. that its intensity is measured by the amplitude or the square of the amplitude of the vibrations by which it is produced, which he considered incomplete when waves of different lengths are compared. The conclusion at which he arrived was that, if two notes were sounded separately, differing by seven octaves, when the tympanal cavity was exhausted or inflated, the force of resistance to the motion of the aural nerve was the same in either case, but that the primary force (that which is lessened by the resistance) was 128 times as great in the one case as in the other; therefore the resistance in the one case might be sufficient to stop the motion of the nerve entirely, i.e. to suppress the note, while in the case of the higher note, the effect on the motion of the nerve, and therefore in the degree of distinctness with which the note is perceived, might be practically inappreciable.

May 1, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

New Fellows elected:

> Rev. G. HALE, M.A., *Sidney Sussex College.*
> Rev. C. SMITH, B.A., *Sidney Sussex College.*
> A. G. GREENHILL, B.A., *St John's College.*

Communications made to the Society:

On the Measurements of an Arc of the Meridian in Lapland. By I. TODHUNTER, M.A.

The object of this Memoir was to draw attention to the numerous errors which have been made, even by distinguished astronomers, in their accounts of the two measurements of an arc of the meridian in Lapland. A comparison of the original authorities on the subject at once detects these errors and supplies the necessary corrections.

Professor MILLER asked whether a zenith sector referred to by the author was the one used at the equator, some faults of which he pointed out; he also enquired whether observations had been made to determine the eccentricity of the quadrants used in the Lapland measurements.

Mr TODHUNTER said the sector was not the same; and that there was no clear information given on the other points.

Mr GODFRAY asked if the toises used in the observations had been compared.

Mr TODHUNTER replied in the affirmative. Some further conversation occurred in which the President (Prof. CAYLEY) and Mr ELLIS took part.

May 15, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

The Treasurer (Dr CAMPION) gave a statement of the financial condition of the Society, which he pronounced to be satisfactory. A vote of thanks, proposed by Prof. BABINGTON and seconded by Prof. SELWYN, was heartily accorded.

Communications made to the Society:

(1) *On Dr Wiener's Model of a Cubic Surface with 27 lines; and on the Construction of a double-sixer.* By Prof. CAYLEY.

I call to mind that a cubic surface has upon it in general 27 lines which may be all of them real. We may out of the 27 lines (and that in 36 different ways) select 12 lines forming a "double-sixer," viz. denoting such a system of lines by

$$a_1,\ a_2,\ a_3,\ a_4,\ a_5,\ a_6,$$
$$b_1,\ b_2,\ b_3,\ b_4,\ b_5,\ b_6,$$

then no two lines a meet each other, nor any two lines b, but each line a meets each line b, except that the two lines of a pair $(a_1, b_1), (a_2, b_2),\dots(a_6, b_6)$ do *not* meet each other. And such

a system of twelve lines leads at once to the remaining 15 lines; viz. we have a line c_{12}, the intersection of the planes which contain the pairs of lines (a_1, b_2) and (a_2, b_1) respectively.

The model is formed of plaster, and is contained within a cube, the edge of which is $= 18.2$ inches; the lines a, b, c are coloured blue, yellow, and red respectively; the lines a_1, b_2, b_5 being at right angles to each other, in such wise that taking the origin at the centre of the cube, the axes parallel to the edges thereof, and the unit of length $= 1.6$ inches, the equations of these three lines are

$$
\begin{array}{lll}
a_1 & x = 0, & y = 0, \\
b_2 & x = 0, & z = 1, \\
b_5 & y = 0, & z = -1.
\end{array}
$$

The model is a solid figure bounded by portions of the faces of the cube, and by a portion of the cubic surface, being a surface with three apertures, the collocation of which is not easily explained.

(2) *On the Tides in a rotating Globe covered by a Sea of constant depth at all points in the same latitude, and attracted by a Moon always in the plane of the equator, supposed either fixed or moving with uniform angular velocity; considered with reference to the tides as they are in nature, and the retardation of the earth's angular motion.*

(3) Also, *On the motion of imperfect fluid in a hollow sphere rotating about its centre under the action of impressed external periodic forces, considered with reference to the phenomena of Precession and Nutation. By* Mr Röhrs.

In the first of these papers it was shewn that by assuming the Moon to be in the equator always, her effect would be greater

than in nature; and by assuming that the sea was relatively at rest close to the bottom and stuck to it, so to speak, the greatest possible effect of roughness of the bottom was obtained. These data being assumed, the problem became a simple but rather lengthy application of Professor Stokes' equations of fluid motion; and the result of the investigation was to shew that the internal friction of the sea was too small to produce a sensible effect; the angle of lagging not being more than $2''$ or $\frac{1}{100\,000}$ for an ocean of which the depth or latitude was supposed to vary so as to give a tidal range nearly on the scale of nature. The retardation due to this small value of λ would not be more than enough to occasion an increase in the length of the day of one second in *one hundred millions of years!* Practically none at all. Mr Röhrs admitted that close to shores and in narrow channels the retarding action of the sea would be greatly magnified, but he thought that this increased action would be more than counterbalanced by the entire absence of tidal retardation in the parts of the globe where no sea existed. Besides, the hypothesis of the sea "sticking to the bottom" gives an amount of retarding force due to the roughness of the sea-bed greater than what could be the case under any circumstances; hence the value of λ so obtained will be a superior limit. One second of retardation in 100,000,000 years would only make an error of 12 seconds in the date of an eclipse observed 2500 years ago; and as the error to be accounted for in eclipses then recorded is about an hour and a half, 450 times 12 seconds is required; therefore tidal retardation cannot, as some have supposed it might, account for this discrepancy.

The discussion of the second problem shews that if a globe, 4000 miles in diameter, composed of a thin crust and imperfect fluid interior, be made to rotate under the action of a force going through its phases in 27000 years, and if w and w' be the angular velocities of the globe at its crust, on the hypothesis of

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the globe being solid throughout, and fluid respectively; and if $w = n \sin pt$, w' will be $= n' \sin pt - m' \cos pt$, where m' will be at least $\frac{1}{35}$th of n, $\therefore n' = n$ nearly, unless μ' the coefficient of fluid friction of the globe's interior be at least 150,000,000 times that of water; and for forces going their phases in 20 years μ', in order that m' may be not more than $\frac{n}{35}$, must be at least $\frac{27000}{20} \times$ this great value, or at least 200,000,000,000 the value of μ' in water. The application of these facts to the phenomena of Precession and Nutation is obvious, and points to a condition of the interior of the globe which is inconsistent with all notions of fluidity; since a deviation, not merely from the value, but from the force of w to the extent shewn above, would not escape observation and detection.

Mr Röhrs observed in conclusion, that he did not know how far, or if at all, he had been anticipated by other persons in the solution of these problems; as the problems present no difficulties of analysis, it was likely he had been, but he was not aware of that fact—his work was entirely his own. He had heard, however, that since his paper had been in the hands of the Society, that is, within the last year or more, Mr Stone, of Greenwich, had announced that tidal retardation was practically insensible in amount.

Professor Stokes thought that it would not be safe to assume (as had been done) the value of the constant which had been determined by himself, for in investigating that case the motion in the fluid had been supposed very slow, so that no eddies were formed, and fluid friction only acted. But in the problem of the tides, as the bed of the ocean would generally be rough, the formation of eddies would be an important element in the matter, and thus the resistance might be much greater than in the case contemplated above.

Mr O. Fisher mentioned that Archdeacon Pratt had replied

to Delaunay's criticisms in some letters published in *Nature;* and read an extract from the last of these. He asked whether it was true that the axis of the fluid nucleus would not be affected by the motion of the shell of the earth, and whether it would not drag upon the fluid interior.

Mr Röhrs replied that it would drag.

Professor Stokes said that a hollow sphere with a *perfect* fluid within would pass over the fluid without dragging, whereas a very viscous fluid would follow that sphere; therefore it was entirely a question of degree.

Professor Cayley enquired whether Mr Röhrs had examined Delaunay's paper, in which he accounted for the difference between the observed and calculated time of ancient eclipses by a retardation leading to an alteration in the length of the day.

Professor Stokes made a few remarks on the mode of attempting the problem in the former of Mr Röhrs' communications.

May 29, 1871.

The President (Professor Cayley) in the Chair.
Fellow elected:

M. Foster, M.A., M.D., *Trinity.*

Honorary Members elected:

Prof. Sir Benjamin Collins Brodie, M.A., F.R.S.

W. B. Carpenter, M.D., F.R.S.

A. R. Clarke, Capt. R.E., F.R.S.

Prof. T. Huxley, M.D., F.R.S.

Prof. Bartholomew Price, M.A., F.R.S.

William Spottiswoode, M.A., Treas. R.S.

Prof. F. W. A. Argelander (Bonn).

Prof. A. Clebsch (Göttingen).

Prof. A. O. Des Cloiseaux (Paris).

Prof. H. Helmholz (Berlin).

Prof. F. Wöhler (Göttingen).

Communications made to the Society:

(1) *On an illustration of the empirical theory of Vision.*
    By Mr COUTTS TROTTER.

Mr TROTTER gave an account of some experiments which he
had made. With one eye he was shortsighted, the other had
the ordinary range of vision. On using spectacles with one
glass concave and the other plane, so as to bring the defective
eye up to the normal range without altering that of the other,
he found some difficulty in judging distances, &c. Thus when
both his eyes had the same focal length, and were thus both
perfect instruments, he did not see so accurately as when one
of them was an imperfect instrument. Hence he contended
that the result of his experiments supported the empirical
theory of vision.

(2) *On a Table of the Logarithms of the first* 250
    Bernoulli Numbers. By Mr GLAISHER.

Mr Glaisher's communication was not of a nature to be
given in abstract.

(PART XIII.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.
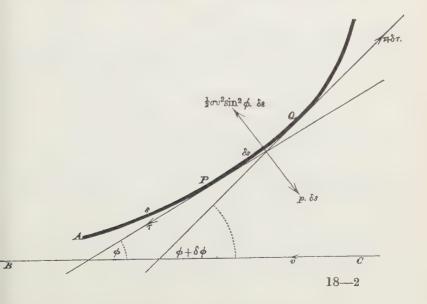
A Theory of the Forms of Floating Leaves in certain Plants. By W. P. HIERN, M.A.

WHEN a flat leaf with flexible margins grows steadily under favourable conditions and floats in running water, let it be supposed that during growth the action between two contiguous portions of an undivided part of the margin is entirely tangential and of the nature of tension, and not submitted either to normal strains or wrenching couples. Let it also be supposed that the vital power of growth as exhibited at the margin at any instant may be expressed mechanically at each point

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by a normal pressure outwards p, which is constant for all points at the instant under consideration.

Thus if APQ be a portion of the curvilinear margin of a leaf exposed to the resistance of the current which is moving with velocity v in the direction CB, σ the density of the water, τ the tension at the point P, $\tau + \delta\tau$ the tension at the contiguous point Q, the arc $AP = s$, $AQ = s + \delta s$, ϕ the angle made by the tangent at P with BC, $\phi + \delta\phi$ that at Q; then, assuming the usual law of resistance due to the current, the element PQ when the power of growth is just balanced, will be in equilibrium under the following mechanical forces:

Tension τ at P along tangent at P in a direction remote from Q,

tension $\tau + \delta\tau$ at Q along tangent at Q in a direction remote from P,

resistance $\dfrac{1}{2}\sigma v^2 \sin^2\phi \,.\, \delta s$ normally inwards,

pressure $p \,.\, \delta s$ normally outwards.

By resolving these forces first tangentially with respect to P and then normally, the following equations are obtained:

$$-\tau + (\tau + \delta\tau)\cos\delta\phi + \left(p - \frac{1}{2}\sigma v^2 \sin^2\phi\right)\delta s \,.\, \sin\frac{\delta\phi}{2} = 0,$$

$$(\tau + \delta\tau)\sin\delta\phi - \left(p - \frac{1}{2}\sigma v^2 \sin^2\phi\right)\delta s \,.\, \cos\frac{\delta\phi}{2} = 0\,;$$

and passing to the limit when $\delta\tau$, $\delta\phi$, δs are indefinitely diminished, it is readily seen that

$$\frac{d\tau}{ds} = 0,$$

$$\frac{d\phi}{ds} = p - \frac{1}{2}\sigma v^2 \sin^2\phi\,;$$

therefore τ is constant for all points, and

$$\frac{ds}{d\phi} = \frac{\tau}{p - \frac{1}{2}\sigma v^2 \sin^2\phi} \;;$$

therefore $\quad \dfrac{p}{\tau} \cdot s = \displaystyle\int \frac{d\phi}{1 - \frac{1}{2}\cdot\frac{\sigma v^2}{p}\sin^2\phi} = \int \frac{d\cdot\tan\phi}{1 + \left(1 - \frac{\sigma v^2}{2p}\right)\cdot\tan^2\phi}$

In the integration indicated by the last expression, two cases arise according as p is greater or less than $\frac{1}{2}\sigma v^2$, that is, according as the power of growth is more or less than sufficient to balance the direct resistance of the current.

In the first case take α a subsidiary angle such that

$$\sin^2\alpha = \frac{\sigma v^2}{2p},$$

then $\quad \dfrac{p}{\tau} \cdot s = \displaystyle\int \frac{d\cdot\tan\phi}{1 + \cos^2\alpha\cdot\tan^2\phi} = \frac{1}{\cos\alpha}\cdot\tan^{-1}(\cos\alpha\cdot\tan\phi)\;;$

no constant is required if s and ϕ vanish together.

Also if x, y be the rectangular coordinates of P, the axis of x being parallel to BC,

$$\frac{dx}{d\phi} = \frac{dx}{ds}\cdot\frac{ds}{d\phi} = \cos\phi\cdot\frac{\tau}{p\,(1 - \sin^2\alpha\cdot\sin^2\phi)},$$

$$\therefore \frac{p}{\tau}\cdot x = \int \frac{d\cdot\sin\phi}{1 - \sin^2\alpha\cdot\sin^2\phi} = \frac{1}{2\sin\alpha}\log\frac{1 + \sin\alpha\cdot\sin\phi}{1 - \sin\alpha\cdot\sin\phi}\;;$$

so $\quad \dfrac{p}{\tau}\cdot y = \displaystyle\int \frac{\sin\phi\,d\phi}{1 - \sin^2\alpha\cdot\sin^2\phi} = -\int \frac{d\cdot\cos\phi}{\cos^2\alpha + \sin^2\alpha\cdot\cos^2\phi}$

$$= \frac{-1}{\sin\alpha\cdot\cos\alpha}\cdot\tan^{-1}(\tan\alpha\cos\phi)\;;$$

no constants are required if x vanishes when $\phi = 0$, and y when $\phi = \frac{\pi}{2}$.

Therefore on eliminating ϕ,

$$e^{\frac{p}{\tau}.\sin a.x} + e^{-\frac{p}{\tau}.\sin a.x} = \pm 2 \sec a . \cos\left(\sin a \cos a . \frac{p}{\tau}.y\right).$$

In the second case, that is, when p is less than $\frac{1}{2}\sigma v^2$, assume

$$\sin^2 \beta = \frac{2p}{\sigma v^2},$$

then $\quad \frac{p}{\tau} . s = \int \frac{d.\tan\phi}{1 - \cot^2\beta.\tan^2\phi} = \frac{\tan\beta}{2} . \log . \frac{\tan\beta + \tan\phi}{\tan\beta \sim \tan\phi}$

$$= \frac{\tan\beta}{2} . \log . \frac{\sin(\beta + \phi)}{\sin(\beta \sim \phi)};$$

$\frac{p}{\tau} . x = \int \frac{\cos\phi . d\phi}{1 - \operatorname{cosec}^2\beta.\sin^2\phi} = \frac{\sin\beta}{2} . \log . \frac{\sin\beta + \sin\phi}{\sin\beta \sim \sin\phi}$

$$= \frac{\sin\beta}{2} . \log . \frac{\tan\dfrac{\beta + \phi}{2}}{\tan\dfrac{\beta \sim \phi}{2}},$$

$\frac{p}{\tau} . y = \int \frac{\sin\phi . d\phi}{1 - \operatorname{cosec}^2\beta.\sin^2\phi} = -\int \frac{d.\cos\phi}{\operatorname{cosec}^2\beta.\cos^2\phi - \cot^2\beta}$

$= \tan^2\beta \int \frac{d.\cos\phi}{1 - \sec^2\beta.\cos^2\phi} = \frac{\sin^2\beta}{2\cos\beta} . \log . \frac{\cos\phi + \cos\beta}{\cos\phi \sim \cos\beta}$

$$= \frac{\sin^2\beta}{2\cos\beta} . \log . \left(\cot\frac{\beta + \phi}{2} . \cot\frac{\beta \sim \phi}{2}\right);$$

after eliminating ϕ, the rectangular equation is found to be

$$\tan\beta = \frac{\pm e^{\frac{\cos\beta}{\sin^2\beta}.\frac{p}{\tau}.y - \frac{1}{\sin\beta}.\frac{p}{\tau}.x} \pm e^{\frac{\cos\beta}{\sin^2\beta}.\frac{p}{\tau}.y + \frac{1}{\sin\beta}.\frac{p}{\tau}.x}}{\pm e^{\frac{2\cos\beta}{\sin^2\beta}.\frac{p}{\tau}.y} - 1}$$

In the case when $p = \frac{1}{2}\sigma v^2$,

$$\frac{p}{\tau} . s = \int \sec^2\phi \, d\phi = \tan\phi, \qquad e^{\frac{p}{\tau}.x} + e^{-\frac{p}{\tau}.x} = \pm 2\frac{p}{\tau}.y.$$

The curves whose equations have thus been determined can be traced either from the intrinsic or rectangular equations; in the case when $p > \frac{1}{2}\sigma v^2$ a series of equal detached ovals is obtained whose longest diameters all lie on one straight line, perpendicular to BC; and as the curvature continually diminishes from the point where $\phi = 0$, to that where $\phi = \pm\frac{\pi}{2}$, the diameters parallel to BC are the least, and those perpendicular to BC the greatest. In the second case when $p < \frac{1}{2}\sigma v^2$ a

(1)

(2)

(3)

pair of equal catenary-like curves is obtained with their convexities opposed to each other, which in the last case, when $p = \frac{1}{2}\sigma v^2$, become actual catenaries.

Those parts of the curves obtained above which correspond to points from $\phi = 0$ to $\phi = \pi$ are alone applicable to the original hypothesis that the resistance due to the current opposes the power of growth; parts corresponding to points from $\phi = \pi$ to $\phi = 2\pi$ are either removed from the influence of the current (when circular arcs are obtained for such portions of the margin), or if subject to the influence of the current (by a slight obliquity in the plane of the leaf, the anterior margin being depressed), would have it so as to assist rather than oppose the power of growth, and the equation for such portion of the margin would be

$$\frac{ds}{d\phi} = \frac{\tau}{p + \frac{1}{2}sv^2 \sin^2\phi};$$

assume

$$\tan^2\gamma = \frac{\sigma v^2}{2p},$$

then

$$\frac{p}{\tau} \cdot s = \int \frac{d\phi}{1 + \tan^2\gamma \cdot \sin^2\phi} = \int \frac{d \cdot \tan\phi}{1 + \sec^2\gamma \cdot \tan^2\phi}$$
$$= \cos\gamma \cdot \tan^{-1} \cdot (\sec\gamma \cdot \tan\phi),$$

$$\frac{p}{\tau} \cdot x = \int \frac{d \cdot \sin\phi}{1 + \tan^2\gamma \cdot \sin^2\phi} = \frac{1}{\tan\gamma} \cdot \tan^{-1} \cdot (\tan\gamma \cdot \sin\phi),$$
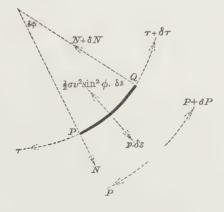
$$\frac{p}{\tau} \cdot y = -\int \frac{d \cdot \cos\phi}{\sec^2\gamma - \tan^2\gamma \cdot \cos^2\phi} = \frac{\cos^2\gamma}{2\sin\gamma} \cdot \log \cdot \frac{1 - \sin\gamma \cdot \cos\phi}{1 + \sin\gamma \cdot \cos\phi};$$

therefore

$$e^{\frac{\sin\gamma}{\cos^2\gamma} \cdot \frac{p}{\tau} \cdot y} + e^{-\frac{\sin\gamma}{\cos^2\gamma} \cdot \frac{p}{\tau} \cdot y} = \frac{\pm 2}{\cos\gamma} \cdot \cos \cdot \left(\tan\gamma \cdot \frac{p}{\tau} \cdot x\right).$$

Suppose that the leaf-margin after acquiring the shape as determined above ceases to be flexible, then the action between two contiguous portions of the margin will not be entirely tangential, but in addition to the tension τ there will exist a normal strain N and a mechanical couple P tending to wrench the margin. Let τ_0, p_0, v_0 be the tension, the power of growth, and the velocity respectively at the time when the margin acquires its shape, then for portions of the margin subject to the opposing resistance of the current,

$$\frac{ds}{d\phi} = \frac{\tau_0}{p_0 - \dfrac{1}{2}\sigma v_0^2 \sin^2\phi}.$$

The new equations of equilibrium will be

$$-\tau + (\tau + \delta\tau)\cos\delta\phi - (N + \delta N)\sin\delta\phi = 0,$$

$$N - (N + \delta N)\cos\delta\phi - (\tau + \delta\tau)\sin\delta\phi$$

$$+ \left(p - \frac{1}{2}\sigma v^2\sin^2\phi\right)\delta s \,.\, \cos\frac{\delta\phi}{2} = 0,$$

and by taking moments about the point P

$$P - (P + \delta P) - (N + \delta N)\,\delta s = 0,$$

and passing to the limit

$$\frac{d\tau}{d\phi} = N, \quad \frac{dN}{d\phi} + \tau = \left(p - \frac{1}{2}\,\sigma v^2.\sin^2\phi\right)\frac{ds}{d\phi}, \quad \frac{dP}{ds} + N = 0.$$

Therefore
$$\frac{d^2\tau}{d\phi^2} + \tau = \tau_0 \cdot \frac{p - \dfrac{1}{2}\,\sigma v^2.\sin^2\phi}{p_0 - \dfrac{1}{2}\,\sigma v_0^2.\sin^2\phi}$$

$$= \tau_0 \cdot \frac{v^2}{v_0^2} + \frac{\tau_0}{v_0^2} \cdot \frac{v_0^2\,p - p_0.v^2}{p_0 - \dfrac{1}{2}\,\sigma v_0^2.\sin^2\phi}.$$

Therefore (see Boole, *Differential Equations*, edit. I. p. 383)

$$\tau = \frac{v^2}{v_0^2}.\tau_0 + \frac{\tau_0.\sin\phi}{v_0^2}\int\frac{\cos\phi\,(v_0^2 p - v^2 p_0)\,d\phi}{p_0 - \dfrac{1}{2}\sigma v_0^2\sin^2\phi} - \frac{\tau_0\cos\phi}{v_0^2}\int\frac{\sin\phi(v_0^2 p - v^2 p_0)\,d\phi}{p_0 - \dfrac{1}{2}\sigma v_0^2\sin^2\phi}$$

$$= \frac{v^2}{v_0^2}.\tau_0 + \sin\phi\int\frac{dx}{ds}.\frac{(v_0^2 p - v^2 p_0)}{v_0^2}.\frac{ds}{d\phi}.d\phi - \cos\phi\int\frac{dy}{ds}\frac{(pv_0^2 - p_0 v^2)}{v_0^2}.\frac{ds}{d\phi}.d\phi.$$

Therefore if p may be considered constant for different points

$$\tau = \frac{v^2}{v_0^2}.\tau_0 + \frac{(v_0^2 p - v^2 p_0)}{v_0^2}\sin\phi\,(x-a) - \frac{(pv_0^2 - p_0 v^2)}{v_0^2}\cos\phi\,(y-b),$$

where a and b are constants;

$$N = \frac{d\tau}{d\phi} = \frac{(v_0^2 p - v^2 p_0)}{v_0^2}\sin\phi.\cos\phi.\frac{ds}{d\phi} + \frac{(v_0^2 p - v^2 p_0)}{v_0^2}\cos\phi\,(x-a)$$

$$- \frac{(pv_0^2 - p_0 v^2)}{v_0^2}\cos\phi.\sin\phi\,\frac{ds}{d\phi} + \frac{(v_0^2 p - v^2 p_0)}{v_0^2}\sin\phi\,(y-b)$$

$$= \frac{(v_0^2 - v^2 p_0)}{v_0^2}.\{\cos\phi.(x-a) + \sin\phi.(y-b)\};$$

$$P = -\int N.ds = -\frac{(v_0^2 p - v^2 p_0)}{v_0^2}\int\left\{\frac{dx}{ds}(x-a) + \frac{dy}{ds}(y-b)\right\}.ds$$

$$= \frac{v_0^2 p - v^2 p_0}{2v_0^2}\{c - (x-a)^2 - (y-b)^2\},$$

where c is a constant.

But since from the nature of the case both τ and P are unaffected when $\pi - \phi$ is written for ϕ, therefore $b = 0$. Thus τ, N, and P are determined; and it is easily seen that the variable portion in the expression for the tendency to break varies as the square of the distance from a point situated in the axis of x.

The theory advanced in this paper applies to leaves whose structure is such that the marginal vigour of growth is the same at all points; and in the case of aquatic plants with floating leaves which may be otherwise organized, some modification of the same theory will be necessary; it applies during the period between the first unrolling of the leaf at the surface of the water and the completion of its growth. Each lobe of a divided leaf must be treated to a separate calculation, and new lobes may be formed at those points where the tendency to break is the greatest. Growth is supposed to proceed steadily, while the leaf is submitted to a suitable external pressure; when violent pressures exist, as, for example, when the current is very rapid after heavy rains or otherwise, growth is probably checked for a time and fissures may be started, or the leaf-margin maintains its shape by the support of its interior which then resists the external pressure, until at a proper time growth pushes forward the margin again, and its form is matured in obedience to the above-investigated laws.

It is further to be noted that the form of the leaf-margin remains the same, even if at different times the power of growth, the direct resistance due to the velocity of the current, and the marginal tension all vary, provided only that their proportional values remain unaltered. It is a tenable hypothesis and by no means improbable that, during much or most of the time when actual growth is taking place and when the velocity of the current is subject to many and various vicissitudes, the plant has the power of adapting its growing efforts to the circumstances just necessary for its development, that is, in the no-

tation of the previous analytical investigation. the quantities p, σv^2, τ, or at all events the first two of them, maintain a constant proportion. The shape of the curve depends only upon the ratio of p to σv^2, and the size depends further upon the proportional value of τ.

It is evident, on the other hand, that neither any one curve nor the system of curves belonging to any one of the above equations nor any portion of it or them can, except in very simple and entire leaves, delineate the whole margin of the floating leaf; for otherwise there would be no means of explaining the divisions, fissures and incisions which are frequent even in floating leaves and which give characters for the definition of species. It does not therefore follow as a consequence of this investigation that all floating leaves grown in a perfectly still water (if such a phenomenon were possible to contrive) are simply circular in outline, though a circular form might be favoured; but it does follow that the several portions of the margin would be circular arcs.

It has before been hinted at, that new fissures (in addition to any previously existing ones) may be made and accounted for by the mechanical action of the current.

It is a matter of common observation that many floating leaves, as for example in *Ranunculus*, vary considerably in consequence of, or in association with, the nature of the stream in which they grow.

At all events the theory discusses the forms which floating leaves would find mechanically suitable for their growth and maintenance, in order that they might dwell free from unnecessary strains and wrenches, and under an equal distribution of their power of growth, which as we know is capable of exerting considerable force under compulsion, but is in general slow and steady.

ANNUAL GENERAL MEETING, OCTOBER 30, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

The following Officers were elected:

President.

Professor HUMPHRY.

Vice-Presidents.

Professor C. C. BABINGTON.
Professor CAYLEY.
Professor ADAMS.

Treasurer.

Dr CAMPION.

Secretaries.

Mr BONNEY.
Mr J. W. CLARK.
Mr COUTTS TROTTER.

New Members of the Council.

Professor MILLER.
Professor MAXWELL.
Mr GODFRAY.
Mr J. STUART.

Communications made to the Society :

(1) *On the Equation which determines the form of the Strata in Legendre's and Laplace's Theory of the Figure of the Earth. By* I. Todhunter, M.A., F.R.S.

The object of this Memoir is to examine various investigations which have been given respecting the equation which occurs in the theory of the figure of the Earth considered as a heterogeneous fluid, and from which it is inferred that the figure must be that of an ellipsoid of revolution. Especially the assumptions on which these investigations rest are discussed. The most general treatment which the equation has hitherto received is shewn to be unsound. Finally a new method is proposed, by which the required result is demonstrated with fewer limitations than have hitherto been employed.

(2) *On a Cirque in the Syenite Hills in the Isle of Skye. By* T. G. Bonney, B.D.

The Syenite hills occupy a portion of the eastern coast of Skye between the Liassic plain of the Strath (through which they have been extruded) and the great Trap district on the north. Though the date of this extrusion is uncertain, it is generally believed to have happened—as did that of the Trap—in Meiocene times. The author stated that he had already described a number of cirques in districts of sedimentary rock (*Quarterly Journal of the Geological Society*, Vol. XXVII. p. 312) ; he was now able to bring forward an instance from the crystalline rocks, in which good examples of such configurations, so far as his experience went, were rare. He considered that the cirques described near the heads of Alpine valleys could not be accounted for by upheaval, or marine erosion, and, by reason of the steepness of their cliffs and the limited space above

them, could not have been excavated by glaciers. In the above-named paper he had brought forward reasons for maintaining that cirques were excavated by numerous rather small streams, acting on rocks, suitably stratified, whose composition and arrangement admitted of considerable meteoric erosion. This cirque in Skye, a double one, viz. Ɛ-shaped, had its cliffs seamed by the tracks of numerous streamlets, each with its little talus of débris resting on sloping glacier-worn rocks below. He held, therefore, that this cirque had been brought to its present state by the action of streamlets, fed by rains; and had to a large extent been pre-glacial, seeing that the floor was ice-worn. Its configuration forbade him to attribute it to a glacier, unless this agent could be invested with a power of eroding vertically.

Mr O. FISHER said the author had shewn the glacier erosion theory would not hold, but he thought that vertical cliffs must necessarily be formed by being attacked from the bottom, and that streams pouring down from above would have a tendency to produce a talus and so to mask rather than to form a cliff. He called attention to the action of the sea as evidenced in the Alps.

Professor MILLER mentioned an instance shewing how slight the excavating power of water often was: at Bamberg, veins of quartz in a rock scarped some 800 years ago, and since then weathered, now protrude only from $\frac{1}{3}$ to $\frac{1}{2}$ an inch.

Professor LIVEING thought that streams could only cut away the bottom of a talus, when they were shot out by an overlying sheet of ice.

Mr BONNEY, in reply, thought it possible that in many cases there had been a pre-existing favourable configuration of the ground, but how that was produced there was now nothing left to tell. He said that streams in flood could move débris from slopes or taluses where they had deposited it when at their usual volume; that it was impossible to lay down a general rule as to either the rate of erosion at any place—(he had no faith

in the application of the Rule of Three to geological chronology),—or whether the transporting force of a stream was greater than, less than, or equal to its denuding force : each case had to be judged by itself. In many cases he thought that at the present day the taluses were increasing on the cirques ; that, however, was only a question of rainfall, strata, and the like : further it must not be forgotten that there is chemical as well as mechanical denudation.

The MASTER of EMMANUEL asked whether there was an observed difference of constitution in the rocks to explain either the floor or a difference in the slope in the walls of the cirque.

Mr BONNEY replied that it was in the case of most cirques difficult to say positively, seeing that their floors were often masked by talus, vegetation, &c., and that in the case of the one described in the paper, in the Syenite, there was no distinction that was conspicuous, although there very probably was some in the chemical constitution of the rock. Differences in the slope of the walls of a cirque in sedimentary rocks were doubtless due to difference in the strata.

Some further conversation took place in which Mr Potter, Mr O. Fisher, and others took part.

November 13, 1871.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society :

(1) *On a double action Pentagraph. By* Prof. CAYLEY, F.R.S.

The machine was exhibited and described.

(2) *On the experimental verification of the laws of the resistances which bodies are subject to, in moving through the air ; and especially on the experiments made by Mr Robins and Dr Hutton with the* Whirling Machine. *By* R. POTTER, M.A.

The experiments made with the Whirling Machine give the resistances which bodies whirled round in the air experience to considerable nicety, and have been considered as correctly giving the resistance which the same bodies respectively would have experienced if they had moved in straight lines. In this paper it was shewn that the centrifugal force communicated to the air, as is continually seen in various winnowing machines and blowing machines, required consideration, and that the experiments must be separated from those in which bodies move nearly in a straight line through the air.

(3) *On the comparison of the results given by the formula for the resistances which bodies experience whilst moving through fluids, investigated in the paper read before the Society on 7th March, 1870, with the experimental results found by Mr Robins and Dr Hutton by means of the Ballistic Pendulum. By* R. POTTER, M.A.

In this paper computations from the mathematical formulæ obtained in a paper read before the Society on 7th March, 1870, were compared with the results found experimentally by Mr Robins and Dr Hutton with the Ballistic Pendulum, to determine the resistance which musket and cannon shot experienced on passing through the air. It was shewn that the computed and experimental results agreed as closely as could be expected in such a subject, and that we may now maintain that the refractory problem of resistances to motion in the air has at length been solved mathematically.

Mr GLAISHER mentioned some observations in which he had been engaged, which shewed that the formula for resistance $p = kv^2$ did not hold in several cases, especially where p was

small, and said that the Aeronautical Society was engaged in repeating the experiments with the Whirling Machine.

Prof. CLERK MAXWELL mentioned that when the Ballistic Pendulum was used, there was a difficulty in accurately obtaining the initial velocity of the shot fired, but in other experiments, viz. those of Mr Bashforth, the shot was fired through frames with cotton threads; these were far better, as then variation in the initial velocity of the shot was avoided.

Some further conversation took place in which Mr Stuart, Prof. Clerk Maxwell, and Mr Potter took part.

<center>*Nov.* 27, 1871.</center>

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

New Fellows elected:

> B. E. HAMMOND, M.A. *Trinity College.*
> W. A. BRAILEY, M.A. *Downing College.*

Communications made to the Society:

(1) *On the Solution of Electrical Problems by the Transformation of Conjugate Functions.* *By* Prof. CLERK MAXWELL, F.R.S.

The general problem in electricity is to determine a function which shall have given values at the various surfaces which bound a region of space, and which shall satisfy Laplace's partial differential equation at every point within this region.

The solution of this problem, when the conditions are arbitrarily given, is beyond the power of any known method, but it is easy to find any number of functions which satisfy Laplace's equation, and from any one of these we may find the form of a system of conductors for which the function is a solution of the problem.

The only known method for transforming one electrical problem into another is that of Electric Inversion, invented by Sir William Thompson; but in problems involving only two dimensions, any problem of which we know the solution may be made to furnish an inexhaustible supply of problems which we can solve.

The condition that two functions α and β of x and y may be conjugate is

$$\alpha + \sqrt{-1}\,\beta = F(x + \sqrt{-1}\,y).$$

This condition may be expressed in the form of the two equations

$$\frac{d\alpha}{dx} - \frac{d\beta}{dy} = 0, \qquad \frac{d\alpha}{dy} + \frac{d\beta}{dx} = 0.$$
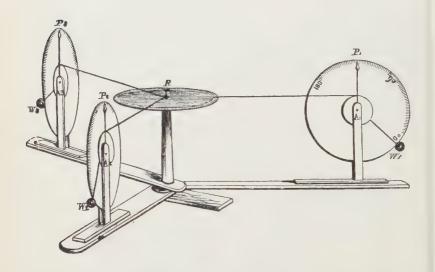
If α denotes the "potential function," β is the "function of induction." As examples of the method, the theory of Thomson's Guard Ring and that of a wire grating, used as an electric screen, were illustrated by drawings of the lines of force and equipotential surfaces.

Professor CAYLEY pointed out a theory of the translation of figures, the small parts of which are the same, which Prof. Maxwell in his paper appeared to be leading up to.

Prof. MAXWELL replied that he had prepared a diagram with the purpose of illustrating this case of the transformation of Conjugate Functions.

(2) *On a machine for illustrating the " Parallelogram of forces."* By J. C. W. ELLIS, M.A.

It consists of three graduated circular laminæ. To the rim of each at the point $0°$ is fixed a weight w. Round the axle of

each is wound a thread. The laminæ are supported vertically on moveable stands. These stands slide along the three spokes of a horizontal wheel and may be fixed at any distance from its centre. These spokes again are capable of moving independently around their common vertical axis so as to take up any angular position with regard to each other. P is an index fixed vertically above the centre of each lamina. It can be easily proved experimentally that the tension of the thread will be proportional to the sine of the angle between W and P. The three threads are all joined to a little ring R so as to rest horizontally on a horizontal circular graduated lamina attached

to the end of the vertical axis of the wheel whose moveable spokes support the laminæ.

Let T_1, A_1, P_1, W_1 be respectively the tension of thread, the centre, the pointer and the weight fixed to the rim of one lamina, and so of the others.

To use the machine—Place a pin through the ring R so as to fix the ring to the centre of the graduated horizontal lamina. Revolve the spokes so that the angles A_1RA_2, A_2RA_3, A_3RA_1 as read off on the horizontal lamina may be any selected. Slide out the vertical laminæ until by the tension of the threads the weights rise so that

$$\angle P_1A_1W_1 = \angle A_2RA_3,$$
$$\angle P_2A_2W_2 = \angle A_3RA_1,$$
$$\angle P_3A_3W_3 = \angle A_1RA_2.$$

But
$$\frac{T_1}{\sin P_1A_1W_1} = \frac{T_2}{\sin P_2A_2W_2} = \frac{T_3}{\sin P_3A_3W_3};$$
$$\therefore \frac{T_1}{\sin A_2RA_3} = \frac{T_2}{\sin A_3RA_1} = \frac{T_3}{\sin A_1RA_2},$$
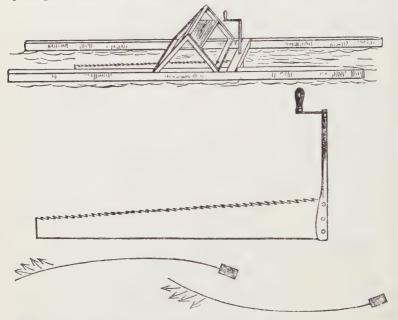
or the tensions of the threads are as the sines of the angles between the other two. Now remove the pin, and the system will be found to be in equilibrium.

(3) *On a mode of propelling vessels.* By J. C. W. Ellis, M.A.

In a fish the tail is the great propelling machine, the office of the fins is merely to guide and balance. It is rather difficult to conceive this action of the tail which is able to drive the body through the resisting medium at such great speed.

The single oar at the stern of a boat as used by seamen, or one at the side as used at Venice or in the Upper Rhine, produces motion in a way not altogether unlike that of the tail of a fish.

The screw of a steamer acts also in a similar way, and perhaps this is the most perfect machine for the purpose of
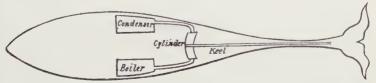
propulsion that can be invented. But as rapid rotatory motion is requisite to drive the screw with effect, the machinery is not of the simplest kind.

A method of propelling has been tried, where motion is obtained by driving water through a tube astern. This, however, is but another form of the screw.

Another method has been proposed where the action of the foot of a water-fowl is imitated.

The present method is one founded on an attempt at a closer imitation of the action of a fish's tail.

The following is the result of some experiments made about two years ago on a lake in Wales. Two wooden pipes about 26 feet long were covered with canvas and tarred: they were fixed parallel about 1 yard apart, and a seat raised above them.

This formed the boat. A crank-handle was attached to a vertical shaft opposite the seat. The lower end of the shaft was about 7 inches below the surface of the water. This end was so contrived that boards of various shapes and sizes to imitate a fish's tail might be attached at pleasure so as to lie horizontally just under the surface of the water. A number of deal boards of various lengths, taper and shape were tried successively, but with not much success. Finally a large steel saw about 8 feet long was introduced. This was so far successful that had the saw been broader, and stiffer at the fixed end, it was clear that fair results might be anticipated. As it was, with very slight exertion, although the boat was very heavy, being nearly water-logged by water having found its way into the pipes, the boat moved forward at the rate of about two miles per hour. The action of the saw seemed to be precisely that of the tail of a fish, lashing from side to side and driving the water astern. When the crank-handle was turned to one side the saw was bent, and in trying to recover itself produced a pressure upon the water partly to one side and partly astern : it was the reaction of this latter portion which drove the boat forward. Any other form of boat would have done equally well, the upright shaft taking the place of the rudder. A pliant steel rod covered with India rubber or gutta-percha in the form of a tail might be even more successful. Within the last few months it has been reported in the newspapers that boats propelled in this way have been tried successfully on some of the American canals.
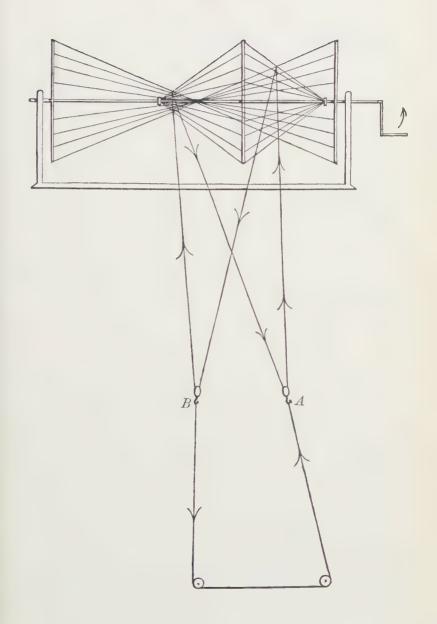
If such a mode of propulsion could be introduced with success, the machinery for driving it might be very simple

indeed. We should not require rotatory motion. The tail might be attached directly to the middle of a cylinder sliding backwards and forwards on a fixed piston-rod, the cylinder and piston-rod forming small arcs of circles, the piston-rod being also the steam-pipe.

(4) *On a method of transforming rotatory motion into rectilinear, so that the rotatory motion remaining constant the rectilinear may be completely controlled and made to vary as to speed—may be stopped or reversed at pleasure. By* J. C. W. ELLIS, M.A.

Two equal cones with their vertices fixed together forming a double cone have a common axis. On this same axis there are two other cones with their bases fixed together so that they form another double cone. All these cones are formed of bars, so that the vertex of the first double cone can lie in the interior of the second double cone. The axis is not attached to the first double cone, but is to the second, so that sliding the axis in direction of its length the position of the cones may be altered with regard to each other. The intersections of the cones form two wheels, the sum of whose radii is constant, but the radii may have any ratio to each other.

An endless rope is passed *over* one of these wheels, round a distant moveable pulley *A*, *under* the other wheel, half round it, then round another moveable pulley *B*, and then *under* the first wheel. The two moveable pulleys may be connected by a rope passing over fixed pulleys.

If the cones be now made to revolve and the double cones be placed symmetrically, the wheels they form will have equal radii and the pulleys *A* and *B* will remain in their positions as the endless rope runs round. But if the common axis of the cones be made to slide, the radii of the wheels will alter and the pulley *A* can be made to approach or recede from the cones with any rapidity according to pleasure, without disturbing the

uniform rotation of the cones. A cam might be made to act upon the common axis so as to slide it in or out according to any given law, and so produce any required motion in the pulley *A*. There are endless varieties of uses to which this device might be applied. It was originally designed in order to reduce the rapid rotatory motion of a circular saw into a slow rectilinear motion in order to bring the timber up to the saw, in such a way that the advance might be instantly stopped or reversed, or might be made slower to suit the nature of the timber employed. Some such contrivance might possibly be useful for ploughing purposes, so as to enable lighter tackle to be used than at present, and to regulate the speed of the plough according to the nature of the ground.

Prof. CAYLEY asked whether the third machine could be made accurate enough to allow it to be applied to the work of a pentagraph, for theoretically it was capable of it.

MONDAY, *Feb.* 12, 1872.

THE PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

It was announced by the President that the Adjudicators of the Hopkins Prize (Professors Stokes, Tait, and Clifton) had awarded it to Professor J. Clerk Maxwell.

It was also announced that Vol. XI. Part 3 of the Society's *Transactions* was now ready, and would be delivered to the members on application.

Communications were made to the Society:

(1) *Further observations on the state of an eye affected with a peculiar malformation.* By THE ASTRONOMER ROYAL.

In this paper the author gave numerical results derived from measurement of the astigmatism of an eye, extending over a considerable period of years, which shewed that during this time there had been a change in the astigmatism.

Mr PAINE observed that he had found from his experience as an optician that about 1 in 100 suffered perceptibly from astigmatism, and described the mode of correcting it by glasses. He said that when astigmatism existed in the crystalline lens it was difficult to remedy it ; but not so when it was in the cornea. It was generally supposed that the astigmatism did not alter with age, so that the Astronomer Royal's observations were of much interest, as they shewed a change.

Professors Miller and Maxwell, Mr Trotter and Dr Latham also made some brief remarks on the subject of the paper.

(2) *The comparison of measures* à traits *with measures* à bouts. *By* Prof. MILLER, F.R.S.

A standard of length in which the measure is defined by the distance between certain points in the surfaces by which the two ends of a material bar are respectively bounded is called a measure *à bouts*. A standard in which the measure is defined by the distance between two fine lines traced at right angles to the axis of the bar is called a measure *à traits*. The methods of comparing with one another two measures of the same kind are well known and need not be alluded to here. But the comparison of a measure *à traits* with a measure *à bouts* cannot be effected so readily. The Astronomer Royal, on being consulted respecting the best method of making such a comparison, recommended the following process. Two copies, *AB, DE,* of the original standard *à bouts* are constructed with cylindrical cavities at their middle points reaching down to the points *C, F* in the axes of the bars, where lines are traced at right

angles to their axes, like the cavities adopted by Mr Baily, at the writer's suggestion, near the ends of the standard yards and their copies. Let s denote the length of the original standard, c that of the copy *à traits*.

Then, the end D of the bar DE being brought into contact with the end B of AB, the distance CF may be compared by microscopes with c. Again, A being brought into contact with E, FC can be compared in the same manner with c. And AB, DE may be compared with s by touch. From the data thus obtained the difference between s and c may be readily found.

The cavities formed in the middle points of the bars must weaken them so as to materially injure their value except for the single operation described.

The same end may however be attained without the necessity of sinking cavities down to the axes of the bars. The object of the Observer is to obtain a visible mark invariably fixed for some hours or some days in the axis of each bar near its middle point.

At the middle of each bar let a right-angled prism be attached with its two smaller faces parallel to the vertical and horizontal faces of the bar supposed to be square. Attach also a small plate of glass, having a fine line traced upon it, in such a position that the line seen by total reflexion in the prism may appear to cross the middle point of the axis of the bar at right angles to it.

The marks thus obtained in the axes of the bars may now be used instead of the lines C, F traced in cavities sunk in the bars, and the operation of comparing c with s will be exactly the same as that which has been already described.

In a Memoir by Steinheil on the construction of a *comparateur* for measures *à bouts* in the 27th Volume of the *Denkschriften der Akademie der Wissenschaften* of Vienna, page 166, it is stated that the probable error of a comparison by touch is 0·00005 millimètres, and that of a single comparison

by microscopes is not less than 0·0005 millimètres. This accuracy is however illusory at temperature 0° C, unless the mercurial thermometer used in finding the expansion of the bar, or an accurate copy of it, is preserved for subsequent use. For Regnault's observations shew that thermometers constructed of different kinds of glass, though in perfect accordance with one another near 0° C and 100° C, may differ 0°·5 C at 50° C, as pointed out by J. Bosscha, jun. (*Archives Néerlandaises*, T. IV.; Poggendorff's *Annalen, Ergänzungsband*, v., 1871, page 465). At $16\frac{2}{3}°C$, the standard temperature adopted in this country, the difference may amount to 0°·28 C, which implies an uncertainty of 0·0045 millimètres in the length of a bronze yard, or nine times the probable error of a single microscopic comparison of two such bars, and ninety times the probable error of a single comparison of two end yard bars by touch.

The necessity for appealing to the thermometer used in the original comparison of a bar, in order to find the length of the bar at any given temperature, may be obviated by observing the expansion of the bar from 0° C to 100° C, and from 0° C to about 50° C, by the original thermometer. The same observations being afterwards made with a second thermometer, even if of different glass, and differing in its readings from the original thermometer at points intermediate between 0° C and 100° C, we have data from which we can deduce the temperature, as indicated by the original thermometer, from the reading of the new one, and thus obtain the true length of the bar at a temperature considerably distant from 0° C.

Professor MAXWELL asked what the value of Whitworth's method was.

Professor MILLER replied that the instrument was so delicate that it was only of use when most carefully handled.

Some further conversation occurred in which Mr Ellis, Prof. Maxwell, and Prof. Miller took part.

MONDAY, *Feb.* 26, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Fellows elected :

> J. W. HICKS, B.A., *Sidney Sussex College.*
> E. H. MORGAN, M.A., *Jesus College.*

Communications were made to the Society :

(1) *On Teichopsia, a form of transient 'halfblindness ;'
its relation to nervous or sick headaches, with an
explanation of the phenomena. By* P. W. LATHAM,
M.D.

The author said that the disturbance of vision referred to in
this paper was a subject which had engaged the attention of Sir
John Herschel, the Astronomer Royal, Dr Hubert Airy, and
many members of the medical profession. He should proceed
to shew that it was one stage of a complaint known under the
name of nervous headache, bilious headache or sick headache ;
the complaint not always accompanied by disturbed vision, but
other disordered sensations being substituted for it, and on the
other hand the disturbed vision not being always followed by
headache ; and he should then endeavour to explain the phe-
nomena. He divided the complaint into two stages, (i) the
stage of disordered sensation, and (ii) the stage of headache.
After quoting the descriptions given by those whose names are
mentioned above as well as by persons who had come under
his own observation, he referred to the causes and condi-
tions under which the attacks were induced. It is to be ob-
served, he said, that all these causes and causes like to them
are of a depressing nature, exhausting the power, and therefore
lowering the tone of the system, putting it out of tune, disturb-
ing the harmony of the functions, and at the same time exalting

the susceptibility of the nervous system. The result was that the power of the ganglia of the sympathetic nervous system to conduct, transfer and radiate the effects of impressions, was no longer controlled by the superior force in the cerebro-spinal centres, and instead of tranquil even harmonious action in the various organs as in perfect health, we had convulsive and painful movements. After referring to the effects of irritation and section of branches of the sympathetic, the next step in his argument was that in the disorder under consideration there was first of all *contraction* of the vessels of the brain (probably the middle cerebral artery), and so a diminished supply of blood produced by excited action of the sympathetic, and that the exhaustion of the sympathetic following on this excitement causes the *dilatation* of the vessels and the headache. This he supported by various cases and comparisons. He next discussed the question, why the disorder might be sometimes unilateral and sometimes bilateral, and lastly, why in some cases there is (i) disturbance of vision without headache following, (ii) disturbance of vision followed by headache, and (iii) headache preceded by disordered sensation, but not by disturbed vision ; all of which he maintained were explicable by the theory which he had advanced.

Prof. HUMPHRY said that he had experienced a sudden attack of hemiopia, which was probably of somewhat similar origin ; that on another occasion he had been conscious of considerable mental disturbance, accompanied by dilatation of a pupil. This he believed due to the eye being accidentally touched by a little atrophine, and that the mental disturbance was merely nervous sympathy.

Mr TROTTER asked whether Dr Latham could suggest any cause for the peculiar complicated figure appearing in this disorder.

Dr LATHAM said he could not explain the special form.

Prof. MILLER stated that he himself had sometimes seen the

zizgag outline described, very faint and shadowy, without any other disturbance of the system ; it lasted about ten minutes.

Dr MICHAEL FOSTER asked whether Dr Latham had had the opportunity of seeing any patient during the attack itself, so as to see how far the blood-vessels and the pupils were affected.

Dr LATHAM said that he had recently seen two cases during the headache, the pupil was then contracted ; but he had not lately seen a case during the time of the peculiar affection of the vision.

Dr M. FOSTER said that though Dr Latham had given a general explanation, he had not brought the explanation sufficiently near to the particular case ; that the special description of teichopsia could only be explained when we knew something more about a very complex matter, the vaso-motor centres of the brain ; he also thought that the phenomena described might possibly be produced by some affection of the circulation in the retina.

Dr LATHAM said he was quite aware that some difficulties yet remained in his explanation—he had indeed thought that the minor cases of the disorder might be produced by some affection of the ophthalmic artery.

The PRESIDENT said that he was not inclined to attribute it to the ophthalmic artery, but rather to the brain, whether vascular or not could not yet be said.
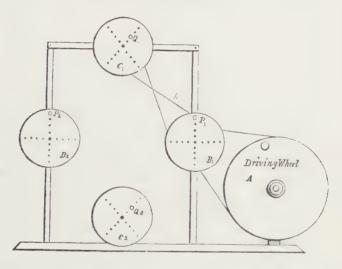
(2) *A machine for tracing Curves described by points of a vibrating String ; namely, curves of the forms*

$$x = a \cos \left(\frac{2\pi}{\tau_1} t + \alpha \right), \qquad y = b \sin \left(\frac{2\pi}{\tau_2} t + \beta \right),$$

and curves of the form

$$x = a \cos \left(\frac{2\pi}{\tau_1} t + \alpha \right) - b \cos \left(\frac{2\pi}{\tau_2} t + \beta \right),$$

$$y = a \sin \left(\frac{2\pi}{\tau_1} t + \alpha \right) - b \sin \left(\frac{2\pi}{\tau_2} t + \beta \right),$$

when α and β are constants, or $\alpha \sim \beta$ a fraction of $t +$ a constant. By J. C. W. ELLIS, M.A.

A driving wheel (A) drives by means of a band a disk B_1. B_2, C_1, C_2 are three other disks equal to B_1. A, B_1, B_2, C_1, C_2 are all in a vertical plane. The centres of B_1, B_2 are fixed in the same horizontal line at a distance of about four times the diameter of the disks. The centres of C_1, C_2 are similarly placed in a vertical line. B_1, B_2 are so connected by rods or otherwise so as to revolve simultaneously. So are C_1, C_2. The axis of B_1 is also the axis of a cone with the vertex pointing from B_1 and revolving with B_1. This cone drives by means of a band (b) the disk C_1, which is attached to a similar cone with its vertex pointing towards C_1. In the disks are bored a number of equidistant holes along the diameters, into which

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can be inserted pegs P_1, P_2 and Q_1, Q_2. P_1, P_2 are fixed at equal distances from the centres in B_1, B_2: Q_1, Q_2 in C_1, C_2.

Now as B_1, B_2 revolve through equal angles in equal times, the line P_1P_2 will always move parallel to itself, and similarly Q_1Q_2.

Hence, if O be the intersection of the lines joining the centres, and P of P_1P_2 and Q_1Q_2; and straight lines in the plane of the disks through O be taken as axes of co-ordinates, the equation to the locus of P will be

$$x = a \cos (m\theta + \alpha_1),$$
$$y = b \sin (n\theta + \beta_1),$$

where b, which can be varied at pleasure, is the distance of the peg P_1 from the centre of B_1, and a of Q_1 from the centre of C_1.

$m : n$ are the velocity ratios of rotation of the disks C_1 and B_1. This ratio may be altered at pleasure by shifting the band along the cones. Either m or n is negative if the bands are crossed. The above equation to the locus of P may be written (as in Donkin's *Acoustics*):

$$x = a \sin \left(\frac{2\pi}{\tau_1} t + \alpha \right), \qquad y = b \sin \left(\frac{2\pi}{\tau_2} t + \beta \right),$$

where t is the time, τ_1 the time of revolution of the disk C_1, τ_2 of B_1, $\alpha = \frac{\pi}{2} + \alpha_1$, $\beta = \beta_1$.

If τ_1 and τ_2 are commensurable then the locus of P is a re-entering curve; if not, not.

If τ_1, τ_2 are nearly in the ratio of two small numbers m and n, the curve though not re-entering after the time mn may be expressed by the equation

$$x = a \sin \left(\frac{2\pi}{m} t + \alpha + kt \right), \qquad y = b \sin \left(\frac{2\pi}{n} t + \beta \right),$$

where k is a small quantity, so that $\alpha + kt$ represents a slow

change of α during the course of each revolution. The curve described, instead of re-entering exactly at the time mn, does so nearly but not accurately, owing to the small change in α, so that it starts on a slightly different course after each interval mn. So that the appearance is that of a curve, slowly changing its character and position, until, if τ_1 and τ_2 are commensurable, it finally returns to its primitive state. If τ_1 and τ_2 are not commensurable it never does so.

All these points can be clearly indicated by fixing a screen so as to cover the disks: having in it two slits at right angles to each other, one in which P_1 and P_2 slide, and the other in which Q_1, Q_2 slide. It is manifest that any point in this screen will trace out the curve

$$x = a \sin\left(\frac{2\pi}{\tau_1}t + \alpha\right), \qquad y = b \sin\left(\frac{2\pi}{\tau_2}t + \beta\right),$$

when a and b can be arranged at pleasure by shifting the pegs, τ_1, τ_2 by shifting the band, and α, β by arranging the position of the disks at starting. A pencil pressed against the screen will trace out the curves.

The form of the curves may also be represented to the eye by piercing a hole in the screen, and placing a strong light behind it; or by means of the electric spark passing between the ends of fine wires, the ends being fixed close together at any point on the screen:

Another way of representing the curve to the eye is this: two screens are provided, one is attached to the disks B_1, B_2 by the pegs P_1, P_2; P_1, P_2 do *not slide* in a slit, but there is a slit extending nearly from P_1 to P_2. Similarly in the screen attached to C_1, C_2 there is a slit extending nearly between Q_1 and Q_2. Light if placed behind can be seen through both screens at once only at the point where the slits cross, namely, at the point P, whose locus is the point we have been treating of.

If to one of these screens a pencil be attached it will trace out on the other screen the curve

$$x = a \cos\left(\frac{2\pi}{\tau_1} t + \alpha\right) - b \cos\left(\frac{2\pi}{\tau_2} t + \beta\right),$$

$$y = a \sin\left(\frac{2\pi}{\tau_1} t + \alpha\right) - b \sin\left(\frac{2\pi}{\tau_2} t + \beta\right).$$

The constants in this can be arranged at pleasure as in the former curve.

It is manifest that if $a = b$ and $\tau_1 = \tau_2$ the curve is reduced to a point, and the corresponding vibration to rest, $i.\,e.$ the composition of the two motions of the paper and pencil produce rest.

This may be taken as an example of interference.

The pencil and every point in the paper are describing the same circle, so that there is no relative motion, and the pencil does not travel over the paper.

The above curve is the epitrochoid, which includes the epicycle as a particular case. By crossing the band we make τ_2 negative, and obtain the equation to the hypotrochoid including the hypocycloid as a particular case.

In this case the curve cannot be reduced to a point, but x may be equal to zero during the motion, or y may be, $i.\,e.$ we may reduce the vibration to either one of two straight lines at right angles to each other.

Prof. CAYLEY mentioned a machine by M. Perigal for describing curves in a somewhat similar manner. Dr Hubert Airy had drawn similar curves with a pendulum.

Mr GLAISHER said that the above machine, exhibited in 1848 at the Royal Society, drew curves of more complexity than that of Mr Ellis. He described the machine and gave a brief sketch of its origin.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Fellow elected :—

    J. W. CARTMELL, M.A. *Christ's College.*

Communications were made to the Society:

(1) *A Monograph of the Ebenaceæ. By* W. P. HIERN, M.A.

The family *Ebenaceæ* was first established by Ventinat in 1799 ; it was revised by Jussieu in 1804 ; and in 1810 it was reduced to its present limits by the great botanist Brown.

In 1837, Geo. Don, in his " General System of Gardening and Botany " vol. iv., gave an account of the whole family as understood by him ; he enumerated about 80 species which he distributed among 8 genera.

In 1844, Alphonse De Candolle monographed the family in the eighth volume of the " Prodromus systematis Naturalis regni vegetabilis," and produced 160 species and 8 genera. Three of these genera were new and several of Don's genera were not maintained.

In the present monograph 5 genera only are recognized, namely, *Royenà* and *Euclea* from Africa, *Maba* and *Diospyros* from various countries, and *Tetraclis* from Madagascar, the last of which is new ; and among these are distributed about 250 species. An account is also given of the fossils that have been published as members of the family, but little confidence is placed in the determination of the genera or family in the case of the great majority of the fossil species, and they are not included in the above-mentioned estimate.

For the purpose of preparing the present paper, the great collections both in this country and on the Continent have been examined.

The economic properties of the various members of the order are fully described.

The head-quarters of the family is India, where the species are numerous, but of the 5 genera which compose the family only 2 (though these are by far the largest genera) occur in the whole of the East Indian regions. Two genera are peculiar to the continent of Africa, and one, a new genus, is peculiar to the island of Madagascar. Not a single species is indigenous to Europe; one however is naturalized in the countries bordering on the Mediterranean Sea; this one species is indigenous to the Steppes-region of Asia and to China and Japan. Tropical Africa, including Natal, has above 40 species; the Kalahari region of South-west Africa south of the tropic and north of the Orange river has 6 species; and the Cape of Good Hope has above 20 species. Australia has about 16 species, none of which occur on the western coast. The Forest region of the Western Continent of Griesbach has only *Diospyros virginiana*, L.; the Prairie region has 2 species: the Californian coast-region none; the Mexican region 8; and the West Indies 6 species. The South American region north of the equator has about a dozen species; the region of equatorial Brazil 9; and the remaining portion of Brazil 14 species. Madagascar has 23 species; the Mascarene Islands 6; the Seychelles 2; Sandwich Islands 2; Fiji Islands 2; and New Caledonia 11 species.

Lists are given, arranged in numerical order, of collections of Ebenaceæ made by the principal botanical travellers.

A chronological list is also given of the published specific names, with references and localities.

The natural orders bearing the closest affinities to *Ebenaceæ* are *Olacineæ, Styraceæ, Anonaceæ, Ternstrœmiaceæ, Sapotaceæ* and

Ilicineæ; a plan is given exhibiting the affinities including these families and others which at a greater distance also bear some affinity to *Ebenaceæ.*

A detailed description of the natural order, the genera, and the species forms the chief bulk of the paper.

An alphabetical list of local names of the species, and diagrams for each genus exhibiting the numbers of stamens in each species, conclude the monograph, which is illustrated by several plates.

(2) *The influence of human degenerations on the production of insanity. By* DR BACON.

The object of this paper was to shew that insanity was a result of degeneration in the race, produced by overcrowded dwellings, vitiated air, insufficient nourishment, interbreeding and the like. The author called attention to the circumstances under which Cretinism existed in the Alps and other places; and shewed that insanity in England was most prevalent in those counties where the agricultural labourers were the worst paid. Thus in Wiltshire 1 in every 12 was a pauper, 1 in every 327 insane: but in Westmorland and Cumberland, where the paupers were 1 in 28 and in 24 respectively, the insane were one in 517 and 543. Hence he held that for the diminution of insanity more must be hoped from measures tending to raise the condition of the people, than from any increase of medical skill.

The PRESIDENT remarked on the importance of the communication, and said that as the town population was increasing at the expense of the rural, it was important to ascertain whether there were any signs of mental degeneration accompanying the asserted physical degeneration among them.

Prof. PAGET enquired whether Dr Bacon had detailed facts in the case of one village which he had mentioned, and made some remarks on the physical degeneration in towns, a larger

percentage of recruits being rejected from towns than from country places.

Dr LATHAM asked whether increasing luxury might not also tend to increased insanity, and this be an equal danger with poverty.

Dr BACON said that no doubt there were evils attendant on civilisation, but that he had founded his remarks on statistics; and that these pointed to the poorest counties being the most liable to insanity.

Dr CAMPION said it must be remembered that the more actively disposed persons left the country, so that the feebler, and those most degenerated, remained behind, and that statistics which did not take account of this could not be trusted.

Some further conversation took place upon the same subject.

(3) *Supplement to a table of Bernoulli's numbers.* **By** J. W. L. GLAISHER, B.A.

April 29, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

The Treasurer (Dr CAMPION) gave a statement of the Society's accounts for the past year, which had been audited by Mr PIETERS and Mr MAIN. The thanks of the Society were proposed by Prof. LIVEING, seconded by Prof. BABINGTON, and unanimously accorded.

Communications made to the Society:

(1) *On certain effects on light on Portland Stone.* *By* F. A. PALEY, M.A.

Mr PALEY described the tendency of Portland stone and the oölites, as Bath, Barnack, Ketton, &c., to contract, in different

degrees, blackness by exposure to the air. He shewed some reasons for doubting if this was due simply to the effects of smoke, and shewed that in all cases, but more markedly in the Portland than in other stones, the blackness was either prevented or removed by the incidence of the sun's rays. Many cases of this were adduced from the buildings in the University, and the black and white portions of St Paul's Cathedral were shewn to be referable to the same causes, or apparently to follow the same law. That the black was not due solely to smut or smoke-marks, was inferred from portions scraped from the blackened surfaces being found to be quite unaffected by soap or solution of soda, and presenting a changed appearance under the microscope. It was suggested, as a question of scientific interest, that the potash or phosphate of lime in some of these stones might, in the course of years, undergo some chemical change analogous to oxidisation; at all events, difficulties were pointed out in the common and obvious conclusion, that the blackening of buildings was in all cases due to the effects of smoke alone.

Mr TROTTER made some enquiries as to the positions of the blackened surfaces with reference to the channels down which rain might run.

Mr O. FISHER had only seen these blackened surfaces in London and Cambridge; and that the black was removed by slight exfoliation of the stone when frozen. The stone which lay about near the quarry was not blackened. Possibly the "quarry water" might have something to do with it.

Professor MILLER thought that some kind of vegetable growth was the chief cause, instancing the black stains common on limestone and dolomite cliffs. The red sandstone of Strasburg Cathedral—though apparently not a favourable stone—was covered with vegetation.

Professor LIVEING mentioned the blackness on parts of the white marble in the floor of King's College Chapel. This he

thought due to vegetation. The action of heat in drawing out-
wards various crystallizing substances in the stone might keep
parts clean that were exposed to the sun

Mr BONNEY thought that the dampness or dryness of the
stone, where sheltered from or exposed to the sun, was the
chief cause ; as favourable or unfavourable to both the growth
of vegetation and the lodgment and chemical action of soot.

Professor BABINGTON said that it was true there was much
vegetation on the stone on the north side of King's Chapel, but
he attributed this blackness to smoke.

(2) *On Faye's method of comparing mètres à traits;
and an improvement of it suggested by* Professor
MILLER, F.R.S.

Diagrams of the instruments were exhibited and described,
and a few remarks were afterwards made on them by Professor
Maxwell.

(3) *On certain lithodomous burrows in the Carbonifer-
ous limestone of Derbyshire. By* T. G. BONNEY,
B.D.

The author referred to two previous communications on the
same subject, and stated that some doubt having been expressed
as to the accuracy of his observations in the most important
case described in one of them, he had again visited the same
neighbourhood. Not only had he confirmed his previous ob-
servations, but he had found a large number of other burrows,
which he described, exhibiting a very fine specimen ; and he
maintained these could not be (as had been said) the work of
marine mollusca, as Pholades. It was very improbable that
they would have lasted so long in limestone rocks; they were

unlike Pholas burrows in shape; they were in positions where it was wholly impossible that Pholades could burrow, as, for example, driven vertically upwards into overhanging slabs of rock; they were at the bottom of valleys of river erosion, such as Miller's Dale and Tideswell Dale, and in one case on a scarp of rock which he was now convinced was artificial. He had some additional evidence for their being the work of snails, and thought that Helix nemoralis and lapicida as well as H. adspersa made them.

Mr NEVILLE GOODMAN described the Monte Pellegrino (Sicily) where the stone all over the mountain is perforated, in situations where the Pholas could not bore, and in rocks which had probably not been submerged since secondary times. He quite agreed, from what he had seen, that these burrows were the work of snails.

Mr O. FISHER asked whether possibly the lime was needed by the snails.

Professor HUMPHRY thought that the mode of making the hole was mechanical, by the odontophore, rather than by chemical action.

Mr O. FISHER exhibited a flint flake from Crayford, which was taken from the old brick earth; it was associated with remains of E. Antiquus and R. Megarhinus, below beds with Cyrena fluminalis and Unio littoralis.

May 13, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

New Fellows elected:

G. F. SAMS, M.A. ⎱ *St Peter's College.*
A. DEY, B.A. ⎰

Communications made to the Society :

(1) *On a method proposed by M. Fizeau for comparing a mètre à bouts with a mètre à traits.* By Professor MILLER.

(2) *On the section exposed at Roslyn Hill Pit, Ely.* By T. G. BONNEY, B.D.

The author stated that hitherto two hypotheses had been proposed to account for the extraordinary collocation of Boulder clay, Cretaceous beds and Kimeridge clay in this pit; (1) which had been advocated by Mr H. G. Seeley and others, that this was the result of faulting ; (2) that, as had been suggested by Mr O. Fisher, the cretaceous beds were a boulder-like mass, that had been dropped in boulder clay times from an iceberg into a depression which it had excavated in the Kimeridge clay. He stated that during the last three years he had frequently visited the pit with a view of testing these theories. He pointed out that if the collocation were the result of a fault we should have in the space of about a hundred yards two corresponding down-throw faults bringing down the boulder clay, and an inner pair of (relatively) up-throw faults for the cretaceous beds, which latter were reversed faults. He also shewed that the lower greensand at the E. end of the pit was not, as had been supposed, *in situ*, and that the boulder clay at the S.E. corner formed a wedge-like mass that ultimately disappeared, allowing the gault to come in contact with the Kimeridge clay. He exhibited plans and sections, and argued that the collocation was in the highest degree improbable on a theory of faulting. There was a third hypothesis possible, that the cretaceous beds had slipped from above the Kimeridge clay into their present position, but though some appearances favoured that, he thought it, on the whole, less probable than the

boulder hypothesis; he only differed from Mr Fisher in think-ing that the valley existed before the iceberg came. He quoted some instances of large included boulder-like masses, especially one recorded by Professor Morris, in Lincolnshire.

Mr O. FISHER expressed his pleasure at the corroboration which his hypothesis had received. He thought the valley could hardly have existed before, because the clay would have formed sides sloping more than the limits of the Kimeridge clay appeared to do. He had, since writing his paper, sometimes thought that the boulder might have been dropped on the top of the Kimeridge clay and crushed its way down into its present position.

Mr BONNEY, in reply, gave reasons for the supposed pre-existence of the valley, and thought it doubtful whether the boulder would be heavy enough to crush out the beds below.

May 27, 1872.

The VICE-PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Communications made to the Society:

(1) *On some properties of Bernoulli's numbers, and, in particular, on Clausen's Theorem respecting the fractional parts of those numbers. By* Professor J. C. ADAMS, F.R.S.

The author stated that the theorem enunciated by Clausen for the determination of Bernoulli's numbers had not been proved by him or by any other mathematician—the memoir proposed by Clausen not having ever been published. The author gave a comparatively simple proof of the theorem. Thirty-one of Bernoulli's numbers are already known; the

author has calculated 22 additional numbers. He also had proved that if n were a prime number other than 2 or 3, the numerator of the n in Bernoulli's number was divisible by n.

Professor CAYLEY called attention to one or two points connected with the paper.

Mr GLAISHER said he had observed that in dividing

$$\frac{B_n}{2} \text{ and } \frac{B_n}{2^n}$$

the period of all the circulation was the same; he had verified this for about 28; he had not yet proved it, but conceived it would follow from Clausen's theorem.

(2) *On some of the symptoms produced by Uræmic poisoning in chronic disease of the kidney. By* P. W. LATHAM, M.D.

The object of this paper was to shew that many of the symptoms, as to the mode of production of which in chronic Bright's disease much discussion has hitherto arisen, might reasonably be explained. That the factors involved were :—

(1)   The impeded passage of the blood through the minute arteries of the system, caused by excessive contraction and hypertrophy of the muscular walls of these vessels, as has been demonstrated by Dr George Johnson.

(2)   The hypertrophy of the heart, developed by the resistance offered to the circulation from the contraction of these small arteries; and

(3)   The impoverished state of the blood, which is the necessary accompaniment of the disease.

The author first dwelt upon the occurrence of paroxysmal dyspnœa or asthma, and after discussing the effects which would be produced if the minute branches of the pulmonary artery were suddenly contracted, and the general symptoms and physical signs which would accompany such an event, he shewed by reference to cases recorded by other observers, and from instances which had come under his own observation, that the theory was supported by facts. He next referred to epileptiform convulsions and uræmic coma, and pointed out why, in some cases, convulsions might occur, and not in others; owing to the predominance of one or other of the above-mentioned factors. He then went on to say, that, although cerebral apoplexy not unfrequently occurred in chronic Bright's disease, where there was atheromatous degeneration of the arteries; yet that, independently of this, the apoplexy might be caused by the velocity of the blood through the minute tubes being retarded, (the velocity through a tube varying as the square of the radius of the section,) and so leading to the formation of a small coagulum of fibrin or a thrombosis. There would then be complete obstruction, and consequently the greatest possible pressure would be brought to bear on the arterial wall and result very probably in rupture. This also, he contended, explained the production of pulmonary apoplexy, and minute apoplexies in the kidneys and spleen, or hæmorrhagic infractions occurring in chronic Bright's disease, where no valvular mischief of the heart or endocardiac disease existed.

Dr BRADBURY thought the symptoms mentioned by Dr Latham were explicable on the supposition that after Bright's disease had set in, thrombosis of the heart had taken place. He described a case of pulmonary apoplexy which he had recently examined, where a large blocking had been caused in the pulmonary artery, and commented upon one or two points in the paper.

Dr LATHAM thought the condition found *post mortem* in the

case quoted by Dr Bradbury supported the theory he had advanced, for as there was no valvular disease of the heart, the obstruction had most probably been caused by some of the minute branches of the pulmonary artery contracting, so as to retard the velocity of the blood through them to such an extent as to allow it to coagulate.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

ANNUAL GENERAL MEETING, OCTOBER 28, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

The following officers were elected :

President.

Professor HUMPHRY.

Vice-Presidents.

Professor CAYLEY.
Professor ADAMS.
Professor LIVEING.

Treasurer.

Dr CAMPION.

Secretaries.

Mr BONNEY.
Mr J. W. CLARK.
Mr TROTTER.

New Members of the Council.

Professor BABINGTON.
Professor STOKES.
Mr HORT.
Mr M. FOSTER.

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Communications made to the Society :

On the form suggested by M. Tresca, and adopted by the Commission Internationale du système métrique, for the Mètres Internationaux. By Prof. MILLER.

The instrument was described.

(1) *On Methods of drawing in Perspective.* By Mr J. C. W. ELLIS.

(2) *On a Method of Levelling (communicated by* Mr ELLIS) *proposed by* Mr W. H. STANLEY.

1. *On methods of drawing in Perspective.*
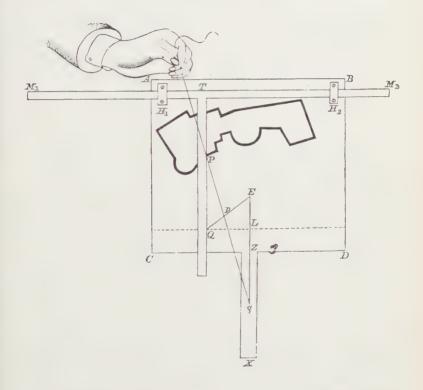
The problem attempted to be solved was this : ' Given the Plan of a building drawn to scale and in any given position with regard to the eye, to cause a pencil by a mechanical arrangement to trace out the corresponding Perspective of the Plan, whilst the operator causes another pencil to follow the outline of the Plan.'

The Mechanical difficulties due to friction, jamming &c. were such, that no satisfactory result was obtainable by the methods employed.

The following step-by-step methods might however be of practical utility, especially in the case of complicated curves.

The Perspective of any object with regard to any given position of the eye is obtained by joining the eye with every point of the object and cutting the cone so formed by a vertical plane. We draw to scale the Plan (whose perspective we require) on a horizontal plane. We assume the eye to be at a height EL above this plane. We take any vertical plane between the plan and the eye, at a perpendicular distance SE from the eye, so that if S be the position of the eye, E is its orthogonal projection on the vertical plane.

$ABCD$ is a Drawing-Board, ZSX a bar of wood fixed to the centre of CD and flush with the Board $M_1H_1TH_2M_2$ a straight bar sliding on the Board through the fixed guides H_1, H_2, so that TQ a ruler, fixed at right angles to M_1M_2 at its central point T, sweeps over the Board and is always parallel to the edges AC or BD

S is a small ring fixed at any required point in ZX. E is another, fixed at any required point in the Board. Any point Q in the ruler will trace out a line (as QL) parallel to CD. ELS is perpendicular to QL. EL (drawn to scale) is the height of the eye above the given Plan, ES the distance from the plane of reference.

One end of a fine thread is fixed at Q, passes through the rings E, S, and is tightened by the hand so as to pass through the point P, where the ruler TQ meets the given Plan. The point p, where the portions of the thread QE, PS intersect, is the 'perspective' of P, or rather of P' the point in the actual Plan to which P corresponds.

That p is the 'perspective' of P, or the point of intersection of the straight line joining S and P made by a vertical plane at a distance ES from the eye, can be seen thus. To obtain the actual position of the eye, we must draw a straight line ES' from E towards us and perpendicular to the paper so that $ES' = ES$. S' will then be the actual position of the eye. Again, the actual position of P (on the assumed scale) is obtained by drawing QP' perpendicular to the paper and on the other side, so that $QP' = QP$. Now if we join $S'P'$, the point when this line meets the paper is the perspective of P'. This point manifestly lies in the intersection of the paper with a plane containing the parallels QP', ES', i.e. it lies in the straight line EQ, and divides EQ in the ratio of ES' to QP', p fulfils these conditions, and is therefore the perspective of P' required.
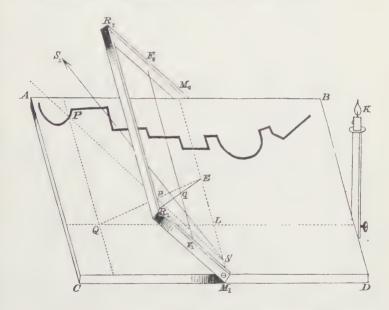
By sliding the ruler, the perspective of every point in the plan, however complicated, may be arrived at; or, in other words, the section of any cone with any vertex and any base may be obtained.

To determine the perspective of any point in an elevation, say n feet above P, draw a straight line through p parallel to the ruler, and where this cuts the thread from S through a point corresponding by scale (as marked off on the ruler) to n feet above P, is the required perspective of the point.

There is a little difficulty, especially in some positions, in marking accurately the point p with a pencil however fine, owing to the effect of Parallax, as both threads cannot lie exactly in the plane of the Board, and also because they must be slightly pushed aside in order to mark with the pencil.

These difficulties may be overcome by a method of shadows in the following manner.
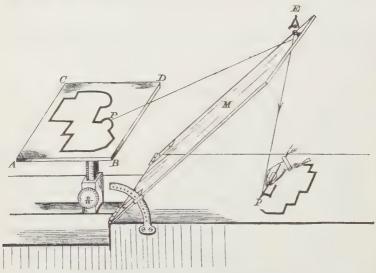
Two rods R_1M_1, R_2M_2 are screwed into the middle points of the opposite sides CD, AB of the Board, and their ends are connected with the rod R_1R_2 so as to form the three sides of a parallelogram. The plane of this parallelogram by revolving about M_1M_2 as an axis may make any angle with the plane of the Board. F_1F_2 is a thread stretched parallel to R_1R_2. E, S are fixed points in the Board in the straight line M_1M_2.

The thread R_1E meets F_1F_2 in q; SS_1 is a thread tightened by hand. K is a candle placed in a vertical plane through q. The shadow of q, namely Q, will trace out a straight line, QL, as in the first method parallel to CD. The shadow of F_1F_2 will be QP parallel to AC, and the intersection of the shadows of R_1E and SS_1, *i.e.* p will be the required Perspective of P.

Another method is represented below. The Board is supported upon a stand which may be slid to any position by run-

ning it in the horizontal groove. It may be raised to any required height by turning the milled head W, and the height is read off on a scale. M is a sheet of transparent glass, which is hinged at its lower end, and may be fixed at any required angle with the horizontal plane. In ordinary perspective this angle
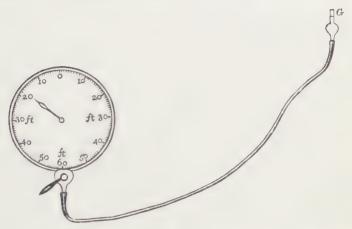
is 45°. The eye E on looking through a small hole in an upright attached to the upper edge of the glass sees by reflexion any point P of the Plan transferred to p, which is the perspective of P corresponding to the eye in the position E with regard to the given Plan. To obtain the elevation of the building, say at a height of 20 feet, we have only got to elevate the Plan by turning the milled head W.

The Perspective of the Plan at any elevation *above* the eye may be obtained by drawing the Plan for the same elevation *below* the eye, and then transposing it, by copying it through the sheet of glass held vertical.

It is manifest by giving various inclinations to the glass we may obtain the corresponding plane sections of a cone of any form.

2. *On a Method of Levelling.*

A gauge, similar to a steam-gauge, is attached to a fine india-rubber pipe 22 yds. long. At the other end is a glass

tube a few inches long with a fixed mark upon it, and just below the glass tube the pipe expands into a small india-rubber bulb. The observer holds the gauge and advances 22 yds., his assistant remains behind and holds the other extremity of the tube, the tube being filled with water. The gauge indicates the pressure due to the difference of Level. By squeezing the bulb the water can always be brought up to the fixed mark in the glass tube G. The observer and his assistant always hold the gauge and the glass tube, say at the height of the eye in each observation. Each lb. of pressure corresponds to a difference of Level of nearly 2 ft. The number of feet corresponding to the pressures are marked upon the gauge, from 0 up to + 60 ft. on the right hand and down to − 60 ft. on the left hand. In the figure the gauge points to + 20, indicating that the observer is 20 ft. below the position of his assistant; had it been − 20, he would have been 20 ft. above him.

As soon as the observer has entered this + 20, in his field-book, the assistant comes on and stands where the observer did, whilst the observer goes on and takes another observation. In order to determine the difference of Level between any two stations he has only to take the algebraical sum of the No. of feet recorded in his field-book.

Instead of the glass tube, a second gauge might be employed, and the *difference of readings* of the gauges entered as the differences of level at each step. In this case the gauges need not be constructed to read below zero.

This method of Levelling would be extremely rapid and sufficiently accurate for most practical purposes. The instrument would be inexpensive, and could be used by an uneducated observer. It could also be employed at night, which would render it useful in military work before an enemy.

By using mercury instead of water, and a very fine flexible tube of copper wire, the readings might be rendered extremely accurate, and the whole weight of the instrument reduced to a very few lbs.

Professor LIVEING communicated a note on Mr Paley's paper, "On effects of light on Portland stone," read at a former meeting. During the summer he had visited Portland, and had found that all the stone there became black on all old exposed surfaces. He had sent specimens of the black part to Mr Berkeley, from the Portland quarries and from King's College Chapel ; who said that they were the early stage of some lichen. Professor Liveing had found this blackness on St George's Church on the top of Portland, but only on surfaces exposed to rain drip—the tombstones also were discoloured. Mr Berkeley had informed him that the lichens in the two cases named above were not the same species. The stones at Portland also occasionally had a red lichen growing on them.

New Fellow elected : H. W. WILSON, Hon. M.A.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society :

(1) *On the advantages of Denison's Gravity Escape-
ment for recording time by Electricity.* *By* Mr
W. KINGSLEY.

The effects of corrosion and irregular impulse are avoided
in this escapement by making the current pass through a por-
tion of the pendulum and gravity arm; the pivot on which
the pendulum is hung, the gravity arms and the banking pins
being insulated, and the terminals of the battery being con-
nected one with the top of the pendulum and the other with
the banking pins, the current is made and broken at the end
of each impulse ; the wear and tear being confined to unim-
portant parts.

(2) *Description of a form of Remontoir Clock invented
by M. Groux.* *By* Mr W. KINGSLEY.

The principle of this remontoir is the making the fly fan
of the remontoir lock on an intermediate detent, so that the
usual concussion and friction are avoided.

Professor MAXWELL commented with approval on the prin-
ciple of the remontoir clock, mentioning one or two conspicuous
defects in existing clocks, and alluding to the difficulty under
which a clockmaker laboured in devising any improvement on
existing patterns.

(3) *On certain facts connected with the wasting and
final disappearance of the Glaciers of North Wales.*
By Mr W. KINGSLEY.

The object of this paper was to shew that the Glaciers of
North Wales were much larger than is commonly supposed,

and more like those of Greenland than of the Alps; filling, as they did, extensive basins, and having little motion excepting at certain points of escape.

That these icefields during their decrease dropped their moraines over large slopes and the hill-sides in such a way as to make it difficult to distinguish the moraine deposit from drift.

Thirdly, shewing in certain cases the manner in which moraines were deposited as the glaciers broke up into smaller ones in the higher recesses of the mountains.

Fourthly, drawing attention to the marks left as evidence of great floods in the valleys during the periods of the wasting of the ice.

Lastly, giving an account of large deposits of fresh-water diatoms in the lakes; these deposits having been made since the glaciers disappeared, but during a cold epoch; and proving that no sea had reached these lakes since that epoch, but giving a means of estimating the time that has elapsed since the glaciers thawed. The whole paper was intended to draw attention to these facts in order that persons might be induced to pursue these investigations to a much greater extent than has hitherto been done.

Professor C. C. BABINGTON spoke very highly of the value of Mr Kingsley's paper, which, he said, had explained to him several things which he had always found much difficulty in understanding.

Mr BONNEY said that Mr Kingsley's paper was a most interesting one, dealing with a particular case of a general problem, the condition of the northern hemisphere in the glacial period. The author appeared to him to have proved that in the earlier part of that period Wales, like Scotland and Scandinavia, had been covered by an ice-sheet—differing thus from Switzerland, where separate glaciers seemed rather to have existed. He was disposed to refer the drift-like scattered

moraine matter, the like of which he had seen in the Alps, not to the retreat of the glaciers after the above period, but to their retreat when, after the great submergence following the ice-sheet period, *glaciers* had formed in the valleys. The enlarged river-channels mentioned by Mr Kingsley were, in his opinion, not due to floods in the ordinary sense of the word, but to the rivers themselves having once been much greater than now, as they might be expected to have been towards the end of the glacial epoch. He had investigated these large river channels in most districts of Great Britain, and in a considerable part of Northern and Central Europe.

Mr O. FISHER was disposed to think that evidence of the ice-sheet period might be obtained even in so flat a country as Cambridgeshire, in certain singular contortions and disturbances of the drift, and superficial deposits, which he could only explain by the pressure of a great mass of ice. He thought that the absence of shells in many of the drifts rendered it unlikely that they were marine, and was disposed to consider that the numerous ice-marks which he had seen in the East of England belonged to an earlier period than those in the West.

New Fellow elected : J. B. LEE, B.A., Sidney College.

November 25, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society :

(1) *On the appearance of an extra digit on the hind limbs and then on both fore and hind limbs in two successive generations ; and its bearing on the theory of Pangenesis. By* Mr N. GOODMAN.

The facts which had come within his personal knowledge, and on which he submitted some remarks, were the following :

Mr Daintree, of Fenton, Huntingdonshire, bought a cow with three well-developed toes on each hind limb besides the two ordinary rudiments which hang behind the foot. This cow was without a pedigree or history. She had a cow calf with the same peculiarity as its dam, which was as well developed as in her case, notwithstanding that the other parent was a normal bull. This cow has had two calves by normal bulls. The first was a cow calf with three toes on each hind limb, but somewhat less developed and less functionally insistent on the ground than in the case of its mother and grandmother ; the second was a bull calf, which had three toes on all four feet. All the toes assumed to be the extra ones have a similar attachment, viz. on the inside of the foot between the internal functional toe and the rudimentary toe on the same side.

Mr Goodman gave a short account of Mr Charles Darwin's theory of Pangenesis, whose main feature is that each individual is made up of organic units, all of which are constantly giving off minute gemmules which float freely through the organism and are transmitted in the generation products to the next offspring, and which are so much in excess of what are required for the building up of the body of the immediate progeny, as to be handed down, many of them, through a great many generations in a latent undeveloped condition. He then applied this theory to explain the facts.

The extra toe might be due, in accordance with this theory, to one or more of the following causes :—

I. Atavism or reversion to an ancestral type.

II. The modification of the proliferous function of certain of the organic units produced by external causes.

III. Correlation of growth, supplementing the last-named cause.

He argued that the abnormality was not due to atavism—

(1) Because it was necessary to travel so far away from the species before a three-toed ungulate was found.

(2) Because the toe did not appear in the position where it must have appeared, on this hypothesis, if received homologies of the toes were reliable.

(3) Because precisely analogous cases had been found in the human subject. Six-fingered and six-toed men were not very uncommon; and as no beast, bird, or reptile had more than five digits on each limb, and yet the extra fingers and toes were definitely human, this evidence was conclusive against the abnormality being due to atavism.

The second cause might operate in two ways. The organic units might be stimulated to throw off more gemmules, or these gemmules might have enhanced affinities. In either case they might attach themselves to any nearly allied growing part between which and themselves there was an affinity resembling their natural one, and so by effecting a double attachment give rise to a double organ. Nor would the extension of the extra abnormal part to a fresh limb, as in the case of the young bull, be unaccountable. For the extra digit, being the direct descendant of a normal one whose organic units had been excessively proliferous, would resemble the parent part not only in structure but in the vigour of its budding function. Thus we should have in the desired animal two energetic manufactures of gemmules instead of one, and in the third generation a still greater excess or avidity in the transmitted gemmules which would manifest itself in a fresh attachment.

But if this were the true explanation of the peculiarity, it would follow that the extra digit, though it had the attachment of a *finger*, would be in reality, as in structure, a *toe*. It is difficult to draw a distinction between the toe and finger of an ox; but as a precisely analogous case had occurred in the human subject, they might safely reason from that. In the case referred to, the extra part had originated in the hands

and been extended to the feet, and in that case the digit was decidedly not a finger but a toe.

They were then driven to the last cause—

III. The correlation of organs. This phrase expressed our ignorance rather than explained it away. Nevertheless some distinctions might be drawn as to the nature of the *correlation*, which was exhibited by the extension of the extra part to the fore limb.

1. It was *not* a teleological correlation, like that which associated the carnivorous teeth with the transverse glenoid cavity for the reception of the hinge of the lower jaw.

2. It was *not* a correlation such as that by which the single occipital condyle was found associated with the segmented jaw in the sauroids.

3. It was not a physiological correlation, since it affected parts containing various structures with various functions—as nerves, bones, &c.

4. It was not an entirely arbitrary (which meant an entirely lawless and unexplained) correlation, as that which determined that white cats with blue eyes should be deaf.

5. It *was* a correlation which obeyed laws and was capable of analogical illustration. It would seem to be due to a force operating in the organism which is best expressed by calling it "polarity," and finds its best analogy in the force which determines that a crystal shall be built up symmetrically around its axes, and that no molecule can be added on one side of these axes without a corresponding one attaching itself to the other. Or perhaps the polarity exhibited in electrolysis affords even a closer analogy. It would be interesting to find that there was latent in the system of the highest vertebrates a force so masked by other dominant forces and exacting conditions as only to appear thus suddenly. and abnormally, yet which was identical with that which determines the shape of the simplest forms of inorganic matter.

Mr Goodman, during the reading of the paper, referred to many abnormalities besides the one which formed the subject of his paper, and many of his deductions were derived from these.

The PRESIDENT made some remarks on the theory of Pangenesis, and read an extract from Darwin, wherein he stated his hypothesis of Pangenesis, and pointed out some of the difficulties attendant upon it, as the difficulty of understanding how such an immense number of gemmules could be contained in a minute ovum, and how they could be transmitted unchanged through successive generations.

Professor MAXWELL spoke of the difficulty of conceiving of chemical molecules in sufficient quantity being packed in these small gemmules.

Dr M. FOSTER spoke of the difficulties in the theory of Pangenesis, especially that mentioned by Prof. Maxwell ; still on that point we have limited positive information as to the size of those chemical molecules, nor do we know how many gemmules were required to be contained in an ovum. It was a morphological limit Mr Darwin was really rather seeking— such a one he (Dr Foster) did not think to hold good, rather was there a physiological one. The "cell" did not always exist as in some low forms of Protozoa. We cannot detect a structure in Protoplasm, still there must be some approach to structure. If you go on dividing it there must be a limit of division, beyond which it cannot continue to live; this therefore suggests a physiological limit. He thought that to the "primary" affinities of gemmules secondary affinities must be added, these must influence also its future fate. With regard to this special problem described by Mr Goodman we must consider (1) How this abnormality first made its appearance ; this he regarded as a case of re-duplication, a tendency to which as to fusion was well-known : this he thought did not disagree with Pangenesis, but that the secondary affinities were

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acted upon. (2) How it was reproduced in the offspring : he thought that the transference of the digit could be explained as Darwin had done, by the correlation of homologous parts ; he thought that these changes were a strong confirmation of the doctrine of homology. He preferred then to regard this result as coming from the action of secondary affinities (he objected to the term polarity) causing re-duplication, and from the homologies of the members.

Prof. PAGET asked how on Darwin's hypothesis it happened that parental defects were often not transmitted.

Dr CAMPION asked whether there was a tendency to re-duplicate on the outside, especially in the thumb of the hand and the little toe.

Mr GOODMAN, in reply, said that he had, in a portion of his paper which he had omitted for curtailment, called attention to the action of primary affinities which determined the association of the gemmules in the generative products, and the secondary affinities by which they were built up in the derived organism ; he shewed that according to Mr Darwin when results were produced by defect of gemmules this could be made up in the next generation by fission of the gemmules. He shewed from instances that the increase was not always on the inside. He gave also some explanation of the muscular system of the abnormality.

Prof. PAGET objected that defects were often made up while excesses were very rarely, so that the chances seemed against the explanation offered by Mr Goodman.

Mr GOODMAN pointed out that it was not only the gemmules from the last ancestors that were transmitted, but from many previous ancestors.

Some further conversation took place on the subject of Pangenesis.

(2) *A Pneumatical Design for saving life at sea.* By Mr W. M. STANLEY. (Communicated by Mr J. C. W. ELLIS.)

Reservoirs of condensed air communicate by means of pipes (similar to gas-pipes) laid throughout the ship. These pipes serve to lead the air into large flexible balloon-like bags stowed away against the ceilings of the various compartments. A handle being turned on deck allows the condensed air to escape from the reservoirs and to expand the bags. Hence in such a case as that of the "London," where the waves filled the vessel, or in the case of a severe leak, the water would be expelled as no pumps could expel it, and a great additional buoyancy given to the vessel. Again, in the case of a fire, the vessel could be partly submerged by opening valves, and the water again driven out by turning on the condensed air.

A better method than that of condensed air would be, perhaps, were a reservoir of water used saturated with ammonia. A steampipe leading to this reservoir would cause the water instantly to part with many times its volume of ammonia and to fill the bags. Or some gas, such as carbon dioxide, in a liquid form, might be employed.

February 3, 1873.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

A SPECIAL general meeting of the Cambridge Philosophical Society was held, when the following alterations were made in the bye-laws :—

To substitute in bye-laws, Sec. vi. § 2 (hour of meeting), *half-past eight* for "half-past seven," and *half-past ten* for "half-past nine."

To make the following new bye-law—

> Residents in Cambridge or the neighbourhood, not being graduates, may be elected Associates of the Society. Each one shall be proposed by three Fellows of the Society, nominated by the Council, and elected by the Society. An Associate shall be elected for a period of three years, and if not then a graduate, shall be eligible for re-election. Associates shall have the privilege of attending the meetings and consulting the books in the Library of the Society.

After an eloquent address from the President on the loss sustained by the death of Professor Sedgwick, whom he justly described as really the founder as well as the ardent promoter of the Society, it was moved by Professor MILLER, and seconded by Professor LIVEING, that an expression of the deep regret felt by the Society at the loss of Professor Sedgwick be recorded on the minutes.

The following communications were made :—

On the Proof of the Equations of Motion of a connected system. By Prof. CLERK MAXWELL.

To deduce from the known motions of a system the forces which act on it is the primary aim of the science of Dynamics. The calculation of the motion when the forces are known, though a more difficult operation, is not so important, nor so capable of application to the analytical method of physical science.

The expressions for the forces which act on the system in terms of the motion of the system were first given by Lagrange in the fourth section of the second part of his *Mécanique Analytique*. Lagrange's investigation may be regarded from a mathematical point of view as a method of reducing the dynamical equations, of which there are originally three for every

particle of the system, to a number equal to that of the degrees of freedom of the system. In other words it is a method of eliminating certain quantities called reactions from the equations.

The aim of Lagrange was, as he tells us himself, to bring dynamics under the power of the calculus, and therefore he had to express dynamical relations in terms of the corresponding relations of numerical quantities.

In the present day it is necessary for physical enquirers to obtain clear ideas in dynamics that they may be able to study dynamical theories of the physical sciences. We must therefore avail ourselves of the labours of the mathematician, and selecting from his symbols those which correspond to conceivable physical quantities, we must retranslate them into the language of dynamics.

In this way our words will call up the mental image, not of certain operations of the calculus, but of certain characteristics of the motion of bodies.

The nomenclature of dynamics has been greatly developed by those who in recent times have expounded the doctrine of the Conservation of Energy, and it will be seen that most of the following statement is suggested by the investigations in Thomson and Tait's *Natural Philosophy*, especially the method of beginning with the case of impulsive forces.

I have applied this method in such a way as to get rid of the explicit consideration of the motion of any part of the system except the co-ordinates or variables on which the motion of the whole depends. It is important to the student to be able to trace the way in which the motion of each part is determined by that of the variables, but I think it desirable that the final equations should be obtained independently of this process. That this can be done is evident from the fact that the symbols by which the dependence of the motion of the parts on that of the variables was expressed, are not found in the final equations.

The whole theory of the equations of motion is no doubt familiar to mathematicians. It ought to be so, for it is the most important part of their science in its application to matter. But the importance of these equations does not depend on their being useful in solving problems in dynamics. A higher function which they must discharge is that of presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning.

In forming dynamical theories of the physical sciences, it has been a too frequent practice to invent a particular dynamical hypothesis and then by means of the equations of motion to deduce certain results. The agreement of these results with real phenomena has been supposed to furnish a certain amount of evidence in favour of the hypothesis.

The true method of physical reasoning is to begin with the phenomena and to deduce the forces from them by a direct application of the equations of motion. The difficulty of doing so has hitherto been that we arrive, at least during the first stages of the investigation, at results which are so indefinite that we have no terms sufficiently general to express them without introducing some notion not strictly deducible from our premisses.

It is therefore very desirable that men of science should invent some method of statement by which ideas, precise so far as they go, may be conveyed to the mind, and yet sufficiently general to avoid the introduction of unwarrantable details.

For instance, such a method of statement is greatly needed in order to express exactly what is known about the undulatory theory of light.

(2) *On a problem in the Calculus of Variations in which the solution is discontinuous.* *By* Prof. CLERK MAXWELL.

The rider on the third question in the Senate-House paper

of Wednesday, January 15, $1\frac{1}{2}$ to 4, was set as an example of discontinuity introduced into a problem in a way somewhat different, I think, from any of those discussed in Mr Todhunter's essay[1]. In some of Mr Todhunter's cases the discontinuity was involved or its possibility implied in the statement of the problem, as when a curve is precluded from transgressing the boundary of a given region, or where its curvature must not be negative. In the case of figures of revolution considered as generated by a plane curve revolving about a line in its plane, this forms a boundary of the region within which the curve must lie, and therefore often forms part of the curve required for the solution.

In the problem now before us there is no discontinuity in the statement, and it is introduced into the problem by the continuous change of the co-efficients of a certain equation as we pass along the curve. At a certain point the two roots of this equation which satisfy the minimum condition coalesce with each other and with a maximum root. Beyond this point the root which formerly indicated a maximum indicates a minimum, and the other two roots become impossible.

New Fellow elected: A. FREEMAN, M.A., St John's College.

February 17, 1873.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

(1) *On the name " Odusseus" signifying " setting sun," and the Odyssey as a Solar Myth. By* Mr PALEY.

This shewed that the name of *Odysseus* or *Ulysses* was more probably connected with δυόμενος ἥλιος, "setting sun," than with ὀλίγος, "dwarf." It was shewn that all the details

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[1] *Researches in the Calculus of Variations, &c.*

of the Odyssey were easily interpreted as connected parts of a solar myth, describing the journey of the sun to the west, and his return, after many struggles and adventures, to his ever-young bride in the east, *Penelope*, the "spinstress," *i.e.* cloud-weaver. The general geography of the Odyssey was noticed, as pertaining rather to Magna Græcia, while the Iliad is essentially Asiatic in its scenery and description. The Cyclops was shewn to be the Sun's eye, extinguished by Ulysses, *i.e.* lost by the Sun when he sinks into the west. The sorceress Circe, and the nymph Calypso, "the coverer," were interpreted as exercising that weird influence over the Sun that is still, by rude races, attributed to magic or to the evil eye. The guidance of Athena, the goddess of dawn, was shewn to be a fitting companion and guide to the Sun in his return. The wreck of Ulysses, and his narrow escape from drowning, was shewn to represent the Sun sinking in the western ocean. Finally, the killing of the suitors with the bow was shewn to be consonant to the usual represen-tation of Apollo and Diana, the sun-god and moon-goddess, who were thought to slay mortals with their deadly arrows. The old Laertes, the father of Ulysses, was compared with old Ti-thonus, the bride of Aurora, and the symbolism explained by the union of the ever old with the ever new.

Prof. SELWYN remarked with reference to the small stature of Odysseus that the sun became larger on approaching the horizon. A passage, however, in the eleventh book shewed a connexion with a solar myth, where Circe tells Odysseus he will feed the herds and flocks of the sun, which were tended by two shepherds (Mercury and Venus). Also a passage in the Iliad, where Jupiter says that all the gods together could not draw him from his seat, but he could lift them with his left hand, the centre of gravity of the system being within the sun.

Mr PALEY said that loss of strength might be meant, and that Plato had referred the myth above named to a solar origin.

Mr JEBB asked (1) how far the allegory was conscious or

unconscious. (2) Concerning the etymology. He thought that the first idea of a great journey might be taken from that of the sun, but he doubted whether the human interest, introduced into the Odyssey, allowed us to bring it into the same category, except so far as that one archetypal idea might underlie it. Therefore he doubted whether the allegory was at all present to the mind of the writer; and so he thought that the journey and its incidents were essentially human; the framework indeed might be supposed suggested by the solar journey, but in the incidents the writer was dwelling on the human side, and so he doubted whether it could be called a solar myth. (3) As to the etymology, the derivation from ὀδύσσετο, as used by Ino, "he with whom the gods were angry," was generally accepted in the best and most critical times of antiquity; and still has the sanction of Curtius, who refers it to ὀδύσσομαι, explaining the ὀ as prosthetic (of ὀβελοι, &c.), thus it would mean the wrathful one, and express the majesty of anger.

Mr PALEY said he thought that the author viewed Odysses simply as a man, but unconsciously followed the tradition.

Mr JEBB asked how far Mr Paley regarded the details, *e.g.* those concerning Circe, Calypso, as supplied by the earlier allegory, or as arising from the mind of the poet when writing on a very simple framework.

Mr PALEY said it was very difficult to say, but he thought that the vitality of the myth would affect them.

Mr FENNELL thought the human interests attached to a myth would cause a very anthropomorphic form to be given to it; and so a hero would grow out of the myth, and action be grouped around him, in accordance with a tendency common in ancient times. He thought Curtius might be wrong about the prosthetic omicron, and that it might be a relic of an old preposition, which still remained in Sanscrit.

(2) *On the identity of the modern Hindu with the ancient Greek ship.* (A model of a Bengalee ship was exhibited.)

This communication explained, by reference to the model of a Hindu (Bengalee) boat, the minute identity in all the details of the mast, sails, tackle, and rudder (or stern-paddle) between the old Greek ship and the modern Indian river-boat. The lowering of the mast, the working of the yard-arm by a man who watches and co-operates with the steersman, and the manner of bringing to shore and fastening the boat, stern ashore and prow to the sea, were illustrated by quotations from Greek poets. The mechanism of the rudder, or paddle, with its " rudder-bands," was explained, with the motion of its own axis produced by the tiller or handle, οἴαξ, the rudder itself being called πηδάλιον. It is a large and heavy timber requiring many men to lift and carry it. Many technical terms in the Greek writers were identified and explained by the model, which appeared to represent the unchanged model that has prevailed for above 2000 years.

Professor MILLER said that the boats on the Boden See were exact models of ancient boats, and described some peculiarity of their rudders.

Professor LIVEING said, with reference to a point Mr Paley had discussed, that he believed the larger junks in the East still had two rudders.

Mr PEARSON (Emmanuel) thought that at any rate in classical times the rudder had been doubled.

The VICE-PRESIDENT (PROFESSOR ADAMS) in the Chair.

Notes on the Hippopotamus. *By* Mr J. W. CLARK.

Mr J. W. Clark exhibited the mounted skeleton, and some portions of the visceral anatomy (preserved in spirit) of the young female hippopotamus, which was born in the Gardens of the London Zoological Society, on January 7, 1872, and died on the following Wednesday. The remarks he made in illustration of the specimen were in substance a *résumé* of the paper read by him before the Zoological Society on Feb. 20, 1872, and printed in their Proceedings.

On the Foraminifera and Sponges of the Cambridge Upper Green Sand. *By* Mr W. J. SOLLAS.

After a description of the Green Sand, and an enumeration of its characteristic foraminifera, the author discussed the origin of its abundant green grains, and indicated that to a large extent these bodies consist of the casts of foraminifera. The included coprolites of the formation were next investigated. Their marked connection with previously existing organic matter was noticed, and it was shewn that this connection characterised the coprolites of various other deposits. Hence was derived a definition for the word "coprolite:" coprolites being defined as "those bodies which have been produced by the phosphatic fossilisation of organic matter, or of the immediate products of its decomposition." The nature of the organisms which furnished this organic matter was shewn, in the case of nodules of hitherto obscure origin, to be spongeous. The sponge-like form of these nodules, the characters and arrangement of their well-marked oscules, and the forms and

disposition of their siliceous spicules, seemed to leave no doubt upon this point. A comparison was instituted between the coprolites of the Green Sand and those of other formations ; in the Lower Silurian of Canada, sponge-like coprolites had been met with. The chalk flints only differed from coprolites in being silicified instead of phosphatised sponges. Ventriculites accompanied both, and in both xanthidia and foraminifera were found. The phosphate of lime which fossilised the green sand sponges might have been derived from the volcanic rocks of Lammermuir, and conveyed to them by the cold current which afterwards eroded the gault and supplied the silicates to infiltrate the foraminiferal casts. Finally, the formation of coprolites appeared to be proceeding in the Chincha Islands at the present day.

Mr PALEY asked how the occurrence of phosphate in Terebratula was to be explained if the coprolites were to be attributed to sponges.

Mr SOLLAS explained that he did not refer all these phosphate nodules to sponges, but to the phosphatization of animal matter.

Professor LIVEING asked what was meant by the nodules being derived from the Gault, for he thought that there was no evidence of the nodules occurring in the Gault.

Mr BONNEY stated that nodules corresponding very closely with those of the Upper Green Sand did occur in the Gault, as for example in the Barnwell pits and in Roslyn pit at Ely. They also occurred high up in the Gault at Folkestone, and were not confined to the base of that formation, although layers existed there, as at Upware and other localities. He congratulated Mr Sollas on the excellent work which he had done with these obscure organisms, and agreed with his results.

Mr PALEY said that the form of the green grains was too regular to make it probable that they were fragments from volcanic rock.

On a Boulder in a Coal Seam, South Staffordshire. By Mr Bonney.

This boulder was found in the 13th coal of the Cannock and Rugeley Colliery—which seam is about three yards thick, and probably about 200 feet above the base of the coal-field, which in South Staffordshire rests on upper Silurian rocks. It weighs 13 lbs. $13\frac{1}{2}$ oz., and is about 19 inches in girth either way, and about $4\frac{1}{2}$ thick. The rock is a very compact grey quartzite, which exactly resembles that of the pebbles in the Bunter conglomerate of Staffordshire. He thought it had been brought entangled in the root of a tree. The difficulty was to find out whence it came. The Bunter pebbles were supposed to have chiefly come from Old Red Sandstone rocks of east Scotland, and to have been originally derived from much older highly altered rocks, probably rather in the north-west of Scotland. The general course of the sediment in both the Bunter and Carboniferous times was from the north-west, and it was probable that the pebble too came from that quarter. The principal difficulty in that supposition was that all the known beds containing similar pebbles to the north-west did not appear likely to have been undergoing denudation in Carboniferous times. Hence the author thought that with our present knowledge the problem could only be stated and not solved.

New Fellows elected:

C. W. Moule, M.A., Corpus Christi College.
C. W. Hitchins, B.A., Sidney Sussex College.

MONDAY, *March* 17, 1873.

The VICE-PRESIDENT (Professor LIVEING) in the Chair.

On an improved Camera Lucida invented by Professor Govi of Turin. By Prof. MILLER.

The peculiarity of the instrument consisted in the use of a transparent film of gold leaf, by means of which the view of the object was improved; but a description cannot be given without diagrams.

Professor LIVEING spoke of the advantage of a methylic aldehyd for depositing silver, and enquired why gold was selected for the above process in preference to other metals.

Professor MILLER said it was, he believed, because of the thinness and durability of the film that could be obtained.

Professor CLERK MAXWELL spoke of some of the advantages of using the films of gold, and commented upon the instrument.

The following were elected Fellows of the Society :—

V. H. Stanton, B.A., Trinity College.

A. P. Humphry, B.A., Trinity College.

J. Dew-Smith, B.A., Trinity College.

C. Yule, B.A., St John's College.

The following were elected Associates of the Society :—

Mr J. Carter, Cambridge.

Mr A. Graham, Cambridge.

Dr Bacon, Fulbourn.

Mr W. J. Sollas, St John's College.

Mr H. N. Martin, Christ's College.

Mr T. Bridge, Non-Collegiate Student.

Mr A. J. Jukes-Browne, St John's College.

Mr P. H. Carpenter, Trinity College.

Mr W. Marshall, Ely.

Mr W. E. Pain, Cambridge.

Mr F. M. Balfour, Trinity College.

April 28, 1873.

The Vice-President (Professor Liveing) in the Chair.

On some so-called "Horite" caves at Beit Jibrín (Eleutheropolis). *By* Prof. Palmer.

Beit Jibrín is the ancient Bethogabra or Eleutheropolis, but the modern name is much older than the Greek appellation and represents the Hebrew Beth Giborím, *the House of Giants,* a name suggestive of the gigantic Philistine inhabitants of Gath : indeed Beit Jibrín is without question the site of Gath, and not only does it fulfil the topographical conditions, but amongst its ruins is one bearing the name Khírbet Ját, i. e. the the ruins of Gath.

Here are some curious excavations which nearly all travellers who have visited them assume to be Horite, and of great antiquity. Dr Robinson, in his *Biblical Researches*, says some contain inscriptions which are the work of casual visitors and do not throw any light on the age or object of their construction : as many of them are written on the domed roof at a height of about 30 feet, and in a totally inaccessible position, it is hard to imagine how this could have been the case.

Professor Palmer read some extracts from his diary, written during a visit to the caves in question, in which he stated that :—

The caves at Beit Jibrín are evidently quarries, though afterwards wrought into their present shape with some ulterior object, such as the formation of granaries, stalls for cattle, &c. The stone is much better at the bottom than at the top, and the method pursued in excavating seems to have been to work downwards, leaving a hole in the roof to give light, and smoothing off the walls as they went on. The last touch of smoothness in some of the walls appears to have been given by cutting out little niches or pigeon-holes and then knocking out the

interstices, by which means a good deal of labour was saved. One cavern is completely covered with blocks which have not been removed in this manner—but the walls of others shew frequent traces of them. The caverns are certainly not earlier than the Christian era, as there are numerous crosses and figures and Cufic inscriptions, the last apparently not earlier than the 4th or 5th century. One of these inscriptions is a personal prayer for the writer as one of the labourers in the cave. Nor could these inscriptions have been done at any other time than during the construction of the chamber. That just mentioned for instance is at a height of 30 feet, and on the arch of the domed roof, so that it could not have been written from a ladder (even if such a thing could have been obtained, and even now tall ladders are unknown in native oriental building), for the writer would have been leaning back in an impossible position, and had to stretch out and carve with mason's tools a distance of 3 or 4 feet on either side. Nor could a scaffold have been used, as it would have been impossible to sling it from the walls, and it must therefore have been built up from the ground, which is absurd for such a mere private and idle inscription. The figures and some of the crosses, especially the geometrical figure (which is done with mason's compasses), imply the same difficulties and, as well as the inscription just mentioned, would require hours of idle work ; just such as might have been done while the steps were still remaining. The inscriptions on the unfinished and finished parts are of the same date and character. The inscriptions consist of formulæ like those of Islam, but without mention of Mohammed ; there is, however, no reason to suppose that the well-known formulæ of Mohammed were other than borrowed from older rites. The Cufic, although posterior to Christianity, is probably anterior to Islam for paleographic reasons, as for instance the word ار سلـحو, the form حو being found for the affixed pronoun as in the Himyaritic writing. The cave in the flat-topped hill is not of any

great extent, and was probably a cistern. Its name *Sendahanna* suggests the Christian title St Anna.

The caves are certainly not *Troglodyte*, and as Ptolemy in the 2nd century mentions nothing of them there is an additional argument, if such were required, for placing them subsequent to the Christian era. One or two appear to have been inhabited, and are covered with black from the smoke of fires, but here the crosses are as much stained as the rest of the wall, neither more nor less. As the floor shews that it is now used for a sheepfold, the smoke is probably an accumulation of years, this particular cave being for some reason more convenient for the herds.

Mr PALEY instanced the Royston cave cut in the chalk, with figures of saints on the sides, but reached by a descending passage, as a somewhat parallel instance.

Mr LEWIS stated that the ear of Dionysius furnished a parallel, but the inscriptions there were now illegible.

Mr BONNEY asked whether there was any resemblance between these caves and those under the Dome of the Rock, and instanced the Royal caverns at Jerusalem as a case of subterranean quarrying.

Professor LIVEING mentioned that the dome-shaped method of quarrying was not very uncommon, as it was often the most economical; and instanced a case in the neighbourhood of Cambridge where the stone had been thus quarried.

Prof. PALMER replied, by briefly sketching out the different kinds of caves in Palestine; he considered that these at Beit Jibrín had no relation with the Dome of the Rock at Jerusalem, for it was quite small. Some caves now used as granaries had probably once been quarries, but many had been excavated for the purpose. He was glad to find that other instances of quarrying in this method could be produced, for he had found that some students of Palestine antiquities had met with difficulties in accepting his views.

On the English sounds of the vowel-letters of the alpha-
bet, on their production by instruments, and on the
natural musical sequence of the vowel-sounds. By
Mr POTTER.

This subject falls under the consideration of the gram-
marians in their studies of the rules which connect spoken with
written language, under the investigations of the physiologists
in their discussions of the structure and functions of the organs
of the voice and articulation, and under those of the natural
philosophers again in their studies of the science of acoustics as
the general theory of sound.

The vowel-sounds are shewn by instruments as well as in
the voice to be infinitely numerous as they slide or glide
gradually from one to another through the whole series or
sequence IEAOU from I (i) to U (u), without breaks or dis-
continuity. Certain sounds of the series are however considered
normal sounds, and are supposed to be represented by the vowel-
letters of the alphabet; though with little unanimity amongst
our grammarians.

The comparison of the speech of different countries is of
course a distinct study, and does not fall generally under the
present subject of discussion.

The author of the paper having had to lecture through many
years on acoustics in the general course of experimental natural
philosophy, using, amongst other acoustical apparatus, Kempe-
len's funnel-shaped instrument and Professor Willis's sliding
tubes producing the vowel-sounds, found the sound of the
English vowel I (i) not to be given by them, though he was
convinced that it was a simple vowel-sound as now used in all
large towns, and no diphthong, as many assert it to be from
provincial pronunciations.

Where opinions differ so much a reference to actual ex-
periment is the only safe alternative, and a representation of

the organs of the voice and articulation as near as can be made in the ordinary materials at the service of the instrument maker is desirable. The author found the metallic free reeds as they are called, with sheet brass tongues fixed at one end, and vibrating freely in rectangular apertures cut in sheet brass plates, to be the most available substitutes for the chordæ vocales (thyro-arytenoid ligaments) of the human larynx, and india-rubber hollow spheres the best representation of the human mouth, as resonant cavities. The reeds being fitted by short tubes to apertures cut in the hollow spheres to represent the fauces of the posterior part of the mouth, and opposite apertures to represent the opening of the lips being cut, the vowel-sounds are produced by compressing the india-rubber shell when blowing through the reed, to make the shell take the same form as the mouth when producing a like sound.

In this manner the vowel-sounds of I (i) as in the word *pipe*, of E (e) as in *peep*, of A (a) as in *papa*, of O (o) as in *pope* and of U (u) as (oo) in *poop*, are readily obtained, and shew the English sequence of the vowel-sounds to be the most philosophical. The slender (a) as in *paper*, the open (a) as in *father*, and the broad (a) as in *water*, are also easily produced as variations in sound of the first letter of the alphabet, and in English are rightly treated as such.

The sound of I (i) is produced when the back aperture representing the fauces is constricted and that representing the lips left open, and then the vibrating current of expired air is divergent within the mouth, becoming slightly rarefied from the first law of motion by which each particle tends to move in a straight line and with a uniform velocity, and experiment also shews this rarefaction to take place. Such a change in the vibrating stream of air we know by the properties of organ pipes and wind instruments generally affects the tone of the sound escaping to the open air.

On the other hand the sound of U (u) as *oo* in *poop*, is pro-

duced when the front aperture representing the opening of the lips is constricted and the vibrating current of air is convergent within the mouth and slightly condensed, as shewn by experiments; so that another character is given to the sound passing the lips, and the vowel-sound U (u) is produced.

The sound E (e) as in *peep* is produced when the constriction is less than for I (i) and somewhat in front of the back aperture.

The sound of A (a) open as in *papa* is produced when the front and back apertures are both open.

The sound of O (o) as in *pope* is produced when the front aperture and part of the shell near it is somewhat constricted.

The sounds A (a) slender and broad are given by slight compressions of the hinder hemisphere for the first and of the front hemisphere for the latter.

The whole of the vowel-sounds in their infinite variations are thus communicated to a vocal note produced in the larynx by the state of the expired vibrating air as it passes through the mouth to the external air.

Professor MAXWELL thought that these experiments could hardly be connected with those of Helmholz and Donders, as the vowel-sounds differed in different nations.

Mr TROTTER shewed that what might be called pure vowels were very numerous indeed, but that in his opinion *i* was not a pure vowel-sound, and he commented upon Helmholz's investigations in the analysis of sounds.

Mr SHILLETO observed that he thought that *i* in English was always a diphthongal sound.

Mr FENNELL asked if it was known which of the vowels had the greatest and which the least intensity, supposing the fundamental note constant.

Mr POTTER said he thought *a* (as in father) had the greatest intensity, *u* (like *oo* in book) the least, and made some further remarks on the subject of the paper.

Mr H. GOTOBED was elected an Associate of the Society.

May 12, 1873.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

On some conditions of reflex action. *By* Dr M. FOSTER.

Goltz observed that, while an uninjured frog, placed in a vessel of water the temperature of which was very gradually raised, made efforts to escape as soon as the water became warm, a brainless frog exhibited no movements, and eventually became rigid in the position in which it was first placed. Yet when a brainless frog was so suspended that the toes or feet only dipped into a vessel of water the temperature of which was gradually raised, the feet were always withdrawn by reflex action when the temperature reached 30° C. or thereabouts. The slower the rise in temperature the longer was the withdrawal deferred, but eventually the feet were always withdrawn however gradual the heating of the water. When the whole of both legs was immersed, no withdrawal took place on gradual heating, and the legs became rigid without any attempt to escape having been made. When the legs were immersed up to the knees only, the results were more or less uncertain. It thus appeared that when a sufficiently large surface of the animal was subjected to a gradual heating, reflex actions which otherwise would have taken place were prevented, though the stimulus was by the increase of surface affected largely increased.

The absence of reflex actions in these cases cannot be attributed to diminution of conductivity in the motor or sensor nerves, or of irritability in the muscles, as these are not diminished at the temperatures in question. The author was at first inclined to regard the facts as an example of the more general law of sensation that when a surface of skin is affected

by a stimulus the sensation is most intense at the junction of the affected and unaffected parts (as when the foot is dipped into hot water). But all attempts to get any similar results with other stimuli than heat failed; and an experiment in which the upper part of the body was raised in temperature while the legs were not affected, shewed a great diminution of reflex action in the spinal cord.

Raising the temperature of the spinal cord would naturally be expected to raise (for a time at least) rather than to lower the reflex excitability—but the author has been led by other experiments to conclude that one has to deal here not with simple rise of temperature but with effects of supplying the spinal cord with blood heated above the normal (and therefore possibly carrying in it abnormal products).

The lowering effect of heated blood is shewn by immersing brainless frogs tetanized with strychnia in water at from 30° to 35° C. In a short time all tetanus disappears, and the animal becomes perfectly flaccid, though both muscles and nerves are thoroughly irritable. On removal the tetanus speedily returns, and may be again removed by re-immersion.

The absence of reflex action of the brainless frog immersed in gradually heated water is due to the fact that the gradually heated water is but a comparatively feeble stimulus for the production of reflex action, and before the skin has become sufficiently affected to call forth a reflex action, the spinal cord has become so lowered by the heated blood that it fails to respond by any movement to the stimulus coming from the skin. But inasmuch as a feebler stimulus is needed to awaken consciousness than to produce a mechanical reflex action, the frog possessing a brain begins to move in the heated water at a very early period; and as each movement increases the stimulating effect of the heated water, the movements soon become very general.

The PRESIDENT in proposing a vote of thanks said that the

subject was one of great interest, and that Dr Foster appeared
to have clearly shewn that the raising the temperature of the
blood had affected the reflex excitability of the spinal chord,
and regretted that Dr Foster had not carried his experiments
further and applied them to mammals. For example reflex action
in human beings in cases of fainting was increased by sudden
cold—again, increase of temperature of blood (as in fever)
lowered the nerve power—this seemed to correspond with the
result obtained by Dr Foster. This depression might result
from wear and tear of system, as had often been suggested ;
but it seemed possible to connect it with the results of Dr
Foster's experiments. Again, it might be possible to discover
in this way something with reference to the treatment of
tetanus, at present so difficult and inscrutable a malady. He
thought it would be well to see how far the nerve power in
frogs with brains was affected by raising the temperature of
blood.

Mr TROTTER enquired if Dr Foster had estimated the
amount of heating produced on the spinal chord when one
leg only was immersed, and whether that required raising to
a higher temperature to produce reflex action.

PROFESSOR MAXWELL mentioned that the effect of cooling
certain nerves had been to quicken the circulation.

Dr FOSTER said that these experiments belonged to a
different class of facts to those which he had described. With
regard to Mr Trotter's question, he had not been able after
several experiments to arrive at any very satisfactory results.
It was very difficult to get the frog properly placed. He did
not think that any very practical result would come with regard
to tetanus, for a bath of high enough temperature to affect the
spinal cord would probably affect the respiratory functions also.
The cause of the lowering of nerve power in fever had yet to be
explained.

On the Rete mirabile of the Narwhal.
By Dr H. S. Wilson.

He divided his remarks into three parts. The first portion consisted simply of the anatomical facts derived from his dissections made of a fœtal Narwhal. The second contained remarks on these dissections as far as they differed from man and from the statements of authors who had investigated the subject. The third part embraced the teleological deductions derived from the facts recorded. In his first portion, after describing the principal sources whence the thoracic rete of the Narwhal derived its constituent vessels, and after pointing out wherein they differed from the arrangement of the same vessels in Man, Dr Wilson proceeded to give a minute account of the position, relations, and structure of the rete itself. He shewed that the rete was divisible into halves, each of which derived its constituent vessels from two sources, that these vessels were peculiar in presenting at their origin the same calibre, being very minute, and consisting of great numbers; that they arose from trunks of the aorta of primary or next to primary calibre, and that, thus, in position, the rete was central to the arterial system. He divided the constituent vessels of the rete into three sets, vasa maxima, v. media, and v. minima, giving their distinguishing characters, and concluded this part with the enumeration of the various structures found imbedded in the substance of the rete. The second portion of his paper had reference chiefly to the discrepancies existing between the notes from his dissections and the statements given by Hunter, Breschet, and Owen, on the thoracic rete of Cetacea in general. In his teleological deductions he attempted to bring the arterial retia mirabilia under headings by dividing them into two great classes, bilateral and axial. The axial he further subdivided into terminal and mediate, and each of these again into complete and incomplete. In commenting on the axial system he

remarked that their probable function was threefold, in some cases to supply a large amount of blood to parts, in others to avoid injury from compression of the vessels, and, in many instances, to check the sudden pressure on nerve centres. In considering the bilateral, after stating that it was found only in Cetacea, he proceeded to give not only Breschet's view of its function as a diverticulum for the storing up of oxygenated blood to be supplied to the circulation during the suspension of respiration, but also the more commonly accepted theory that it is a diverticulum protective against over pressure of blood in the circulation. He inclined to believe that its function embraced both theories. After noticing some deductions derived from the peculiarities in the origin, size, and relations of the constituent vessels of the rete, he concluded with a tabular view of the vessels of the thoracic rete, and of the divisions he had proposed for the arrangement of the various forms of retia mirabilia.

The PRESIDENT, in inviting remarks, enquired what became of the outgoing currents of blood from the rete mirabile, as to how it was distributed.

Dr FOSTER spoke in praise of the paper, and regretted that the specimen was not in a more favourable state for examination ; he doubted whether the vasa could be used as reservoirs for oxygen, for the blood at the temperature of the body would speedily oxidate itself with its own oxygen.

Mr TROTTER did not quite see the force of Mr Foster's objection, for this process was the function of the oxygen.

Dr WILSON said the blood returned by the same way as it came. He thought that the storing oxygenated blood was not the *sole* function of the rete ; there must, in his opinion, be some other function, for the seal did not present a rete. What this function was could not yet be settled.

New Fellow elected : C. TAYLOR, M.A., St John's College.

May 26, 1873.

The VICE-PRESIDENT (PROFESSOR CAYLEY) in the Chair.

On curves of the fourth degree. *By* F. W. NEWMAN (communicated by Mr J. STUART)

The curves spoken of in this communication were classified according to their symmetry and the number of their axes.

Prof. CAYLEY said that he considered that the best method of classifying quartic curves was according to the quartic cones of which they were sections. Quartic cones might be divided into singular and non-singular forms; and non-singular forms might be considered to belong to the same species if they could be transformed continuously into one another without passing through a singular form.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

(1) *On the Mechanical Means for obtaining the real roots of Algebraical Equations.* By J. C. W. ELLIS.

The general equation

$$A_0 x^n + A_1 x^{n-1} + \&c. + A_{n-1}x + A^n = 0$$

having been converted (by putting $\cos \theta$ or $\dfrac{1}{\cos \theta}$ for x) into

$$B_n + B_{n-1} \cos \theta + B_{n-2} \cos 2\theta \ldots + B_0 \cos n\theta = 0,$$

various methods were shewn and illustrated by means of models for finding the values of θ.

I. Wheels whose diameters were as $1 : 2 : 3$, &c., were connected together by cogs or straps. Long arms were fixed diametrically on their faces, which carried sliding weights. The values of θ corresponding to positions of equilibrium were read off on a dial and gave the roots of the second equation.

II. Another method was by hingeing a number of rods together of variable lengths and causing them to revolve through angles in the ratio of $1 : 2 : 3$, &c., round a fixed point in the first rod. This was effected by means of an arrangement of fixed and moveable pullies, when a pencil in the last rod passed through a fixed line determined by the constant in the given equation. The angle revolved through by the first rod was a root of the equation.

In the discussion that followed, Mr GLAISHER, of Trinity College, went briefly into the history of this class of mechanical

inventions, stating that he had met with descriptions of several in the Philosophical Transactions of the Royal Society, and elsewhere. He then put some questions to Mr Ellis respecting the degree of accuracy possible to be attained by such a machine.

Professor MAXWELL described a machine which he had seen at the meeting of the British Association this year at Bradford for shewing the extent and action of the tides.

(2) *Graphic representation by aid of a series of Hyperbolas of some Economic Problems having reference to Monopolies.* *By* Mr A. MARSHALL.

The price at which a given amount of any commodity can be disposed of in any market is determined by the circumstances of the buyers. If this amount be measured along Ox and this price along Oy, there is thus determined a value of y corresponding to each value of x; and the locus of the points so obtained may be called the demand curve: let its equation be $y = F(x)$. So if y be the price at which an amount x of the commodity can be produced for the market (x, y) is found, the locus of which may be called the supply curve: let its equation be $y = f(x)$. This method of expressing the problem of value has been known certainly for 35 years: an intersection of the two curves has been explained as giving the " average price " about which Adam Smith proved that the " market price " will oscillate. But it has not been pointed out that, under some circumstances, there may be more than one point of intersection, and that Adam Smith's arguments apply only to the circumstances of every alternate point. Only at every alternate point of intersection can the exchange value remain in *stable* equilibrium: at the other points it is in *unstable* equilibrium.

If an individual has the monopoly of the supply of the commodity in the market, his immediate interest will, of course, lead him to determine x so that $x\{F(x) - f(x)\}$ shall be a maximum. Let the curve $y = F(x) - f(x)$ be traced, whether by direct inductions or otherwise, on a paper on which are already lithographed a series of rectangular hyperbolas having Ox and Oy for asymptotes. It will then be obvious by inspection for which of two amounts that the monopolist may throw upon the market—or, which is the same thing, for which of two prices that he may demand—he will obtain the greatest total nett profit. Many striking results can thus be obtained in cases in which the curves cut one another more than once.

This mode of representation of the problem of monopolies is elastic, and lends itself to the treatment of some complex hypotheses. Specially important results will present themselves, if the assumption be introduced that the monopolist is willing to undergo some abatement of his claims, when, by so doing, he can confer great benefit on the consumers.

(3) *A Machine for constructing a series of Rectangular Hyperbolas with the same Asymptotes.* By Mr H. H. CUNYNGHAME.

This machine was intended for the purpose indicated in the last paper.

ANNUAL GENERAL MEETING, *October* 27, 1873.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

The following officers were elected:

President.

Professor C. C. BABINGTON.

Vice-Presidents.

Professor LIVEING.
Professor MAXWELL.
Mr PALEY.

Treasurer.

Dr CAMPION.

Secretaries.

Mr J. W. CLARK.
Mr TROTTER.
Mr J. BATTERIDGE PEARSON.

New Members of the Council.

Professor HUMPHRY.
Professor CAYLEY.
Professor HUGHES.
Mr ELLIS.
Mr GOODMAN.
Mr S. S. LEWIS.

PROFESSOR HUMPHRY made a communication on certain depressions in the parietal bones of the skull of an Orang and in Man. He showed the skull of an Orang which had been lately presented to the Anatomical Museum by Mr Vores of Caius College, in which these depressions exist. They look as if the bone had been indented on either side of the sagittal suture by the pressure of the finger, the surface being quite smooth and the edges of the depressions bevelled. There was no corresponding alteration in the contour of the interior, the bones being simply thinned at the part. He had not met with a similar abnormity in any other instance of an animal, but had seen it a few times in man, and showed two skulls from the Museum in which it was present, the outer table of the skull being depressed in a considerable area of each parietal bone, and the skull at the parts being quite thin. The remaining bone-structure was healthy, and there was no reason to attribute the condition to disease of any kind or to accident. The appearance and the symmetrical position of the depressions were against both these suppositions. Neither did it seem possible to account for the depressions by any kind of pressure that was likely to occur. Professor Humphry thought they were probably due to a deficiency in the early formative processes in consequence of which the bone had not been produced of proper thickness at these parts, but he could not in the least explain why such deficiency should occur.

A paper was read by C. YULE, B.A., late of St John's, Fellow of Magdalen College, Oxford, "On the Mechanism of opening and closing the Eustachian Tube." In the first part of this paper the arguments in favour of the Eustachian tube being normally closed were reviewed, in consequence of the contrary view having been again revived by Dr Cleland, and some new ones added. The chief point brought forward, however, was the undoubted voluntary power possessed by Mr Yule over his

own Eustachian tube, by which he was able to open and close it at pleasure. When the tube is thus opened, he described the noises made in singing, breathing, &c., as being much intensified, and during loud singing quite unbearable. In order to complete the reasoning logically, the sensations heard in the ordinary ear with a closed Eustachian tube were compared with those heard in the other ear when the tube was kept patent by means of a catheter adapted to the purpose; and in the latter case the modification of hearing was exactly the same as when the tube was voluntarily opened. The second part of the paper was devoted to examining the mechanism of the tube. During the opening of the tube the following points were observed. The soft palate was unchanged in position and form, and hung flaccid; this was important, showing that the tensor and levator palati did not participate in the action; the tongue was not raised, but the only observable change was the approximation of the posterior pillars of the fauces. The reason of this is as follows: The Eustachian tube presents at its inner margin a bluff mass of cartilage which ordinarily occludes the tube; to this is attached the tendon of the salpingo-pharyngeus; below the latter muscle is attached to the palato-pharyngeus, which above arches over to meet its fellow of the other side. At rest the direction of the salpingo-pharyngeus is such as to press the mass of cartilage *into* the tube, but when the palato-pharyngei contract, the insertions of the salpingo-pharyngei are carried inward and a new direction given to these muscles, such that when they contract they tend to draw the lobe of cartilage *out* of the lumen. As the posterior pillars of the pharynx are chiefly made up of the palato-pharyngei, this explains their approximation when an effort is made to open the tube. The clicking sound heard at the commencement of the act of swallowing was pointed out to be due to the separation of the walls of the Eustachian tube.

November 3, 1873.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr F. J. CANDY, M.A., Professor of Mathematics, &c. in the University of Bombay, read a paper containing a description of a new Physiological alphabet, devised by himself, to represent the various consonant- and vowel-sounds of the human voice by a series of symbols formed so as to be analogous to the different positions assumed by the mouth, palate, &c. in expressing them. By means of eleven consonant- and three vowel-forms, each admitting of several small modifications, Mr Candy stated his belief that he had included all possible sounds of which any language is susceptible: and illustrated his position by examples taken from the dialects of Hindostan.

The paper was intended as a sequel to one read by him before the Society, on the same subject, May 25, 1857 : an analysis of which is given in the " Proceedings " of that date.

No discussion on the merits of the invention followed, it being thought that the system was in too crude a state to be of practical utility.

November 17, 1873.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr SEDLEY TAYLOR read a paper " On a suspected forgery in the Vatican manuscript record of the trial of Galileo before the Inquisition."

The preamble of the sentence pronounced in 1633 contains an enumeration of the grounds on which the Inquisition based their verdict of guilty. The existence of unquestionable discrepancies in the face of this document points to a conflict of

evidence only superficially smoothed down—not brought to a definite issue—by the Tribunal. The most serious of these discrepancies relates to an inhibition which the Court asserted had been formally delivered to Galileo by the Commissary of the Holy Office in 1616. From a detailed comparison of Galileo's letters and published works with the contemporary records of the Inquisition, Mr Taylor argued that the documentary evidence on which the judges relied as establishing this point was a fabrication designed to insure the conviction of the accused. He pointed out in conclusion that, if this view be admitted, Galileo must be held entitled, even on the severest view of his legal obligations towards the ecclesiastical authorities, to an absolute acquittal on all the charges.

After a few remarks from Mr GOTOBED, the discussion closed.

Professor SELWYN exhibited a combination of two hoops, united by a straight rod, on which the inner hoop moved, the rod carrying a ball in the centre : the whole being designed to represent the Sun, the orbits of the Earth and Venus, and the conditions under which a transit of Venus between the Earth and the Sun becomes possible.
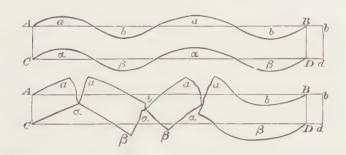
December 1, 1873.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On the Inequalities of the Earth's Surface viewed in connection with the secular cooling. By Mr O. FISHER.

This paper assumes that the elevations and depressions out of which the inequalities of the earth's surface have arisen, are

due to lateral pressure owing to the contraction of the heated interior and consequent wrinkling of the crust to accommodate it to the diminished nucleus.

Let $ABCD$ be a layer of rock of unit of width, length l, and depth k. And suppose the abutments at AC and BD to approach each other through the space le, where e is a small

fraction. Then the layer of rock in question would take some new form, as one of those given in the figure, or any other whatsoever possible.

Call AB "The datum level." Let a, a, &c., be the areas formed by the upper curved line above AB, and b, b, &c., the areas formed by the same line below AB.

In like manner let α, β be similar areas for the lower datum level CD. Then the space included between the curved lines must be equal to

$$Ab\,Cd = kl\,(1 + e).$$

It is also evidently equal to

$$ABCD + a + a + \&c. + \beta + \beta + \&c.$$
$$- b - b - \&c. - \alpha - \alpha - \&c.$$

or, denoting the sums of the quantities similarly situated by the symbol Σ, we get

$$kl\,(1 + e) = kl + \Sigma\,(a) - \Sigma\,(b) + \Sigma\,(\beta) - \Sigma\,(\alpha).$$
$$\therefore kle = \Sigma\,(a) - \Sigma\,(b) + \Sigma\,(\beta) - \Sigma\,(\alpha). \qquad (1).$$

Since the pressure is supposed to take place in a horizontal direction, it will not have any direct effect to raise the centre of gravity of the portion of the crust under consideration; so that, if the layer in question rest upon a liquid substratum, we may expect some portions of the disturbed crust to dip into the superheated rocks. But in that case a corresponding volume of such subjacent rock must rise into the anticlinals.

Hence $$\Sigma(\alpha) = \Sigma(\beta).$$

And the equation becomes

$$kle = \Sigma(\alpha) - \Sigma(b).$$

Extending the inquiry to any *area* of the surface of length l and width w, the equation becomes

$$2\,klwe = \Sigma(A) - \Sigma(B),$$

where $\Sigma(A)$ and $\Sigma(B)$ are the volumes of the elevations above, and of the depressions below, the " datum level."

The whole surface of the globe being next taken into account, the relation becomes,

$$Area\ of\ the\ Globe \times 2\,ke = \Sigma(A) - \Sigma(B).$$

It is important to understand what is meant by the "datum level." It is an imaginary surface, which occupies the position which the surface of the crust would occupy at the present time, if it had been perfectly compressible, so that no corrugations would have been formed in it by lateral compression. For it would in that case have become simply more dense, without being disturbed in position.

The above relation is applicable to the earth's surface, although that is not strictly regular in its general form, and may contain local elevations and depressions affecting its mean figure,—that is, its mean figure as uninfluenced by lateral compression. For these inequalities, though of small amount as compared with the dimensions of the globe, may be large in

comparison with the quantities of which we have to take cognizance in this investigation. Its truth in no way depends upon the arrangement of the disturbed rocks, nor upon the time at which successive movements have taken place, nor upon the alternate elevations and depressions which have at different times affected any given region. It includes every effect of subsequent denudation, from whatever cause, and to whatever amount. In short, it is perfectly general, so long as it is strictly interpreted. But it does not take account of elevations or depressions of regions of the surface arising from unequal contraction in a radial direction, if their result should be to cause a defect of parallelism between the datum level and the surface of the ocean, to which all our measurements must be in practice referred. However, it does not necessarily follow that contractions in the radial direction will cause depressions in the ocean-bed accompanied with a corresponding increased depth of water. For instance, the defect from a true circular form in the equator affects the surface of the ocean, to which the measurements of geodesy are always referred, so that we do not get an additional mile depth of ocean at the extremity of the shorter radius.

If the earth had cooled as a *solid* body, the outer layers at any epoch having attained their complete amount of contraction sooner than the interior, would have been too large to fit the interior after the cooling had proceeded further. They would therefore have become corrugated. But in this case the corrugation would have necessarily taken place wholly in an upward direction; and there could be no places where any portion of the surface could have become depressed below the datum level. Hence upon this hypothesis we may introduce into our datum-level equation the supposition that $\Sigma(B) = 0$. And it becomes $\qquad$ *Area of the Globe* $\times 2\,ke = \Sigma(A)$.

A little consideration will give the following geometrical relation :

The volume of the Sea above the datum level = the area of the whole surface of the globe × the depth of the datum level below the sea level − the volume of rock displacing water between those levels.

Assuming then that the continents have been shaped out of the master elevations, and that the oceans indicate the positions of the master depressions, and that both are ultimately due to lateral pressure, an estimate of $2\,ke$ for the whole globe is obtained from the above relation upon the following data :—

(1) The area of the ocean is 146 millions of square miles.

(2) That of the land is 51 millions.

(3) The *mean* depth of the ocean is three miles.

(4) Its deepest parts are about four miles.

(5) The mean height of the land is 900 feet (as shewn by Mr Carrick Moore)[1].

From these data, as a probable value, $2\,ke = 9504$ feet, which appears more likely to be too small than too large.

The meaning of this in plain language is, that if all the inequalities of the earth's surface were levelled down, they would form a coating 9504 feet thick over the whole globe above the datum level; the datum level being such a surface as has been already defined.

Having thus obtained a value for the thickness of the coating which all the inequalities of the earth's surface would form, if levelled down, a measure of the same thing is sought on physical grounds. For this purpose Sir W. Thomson's paper " On the Secular Cooling of the Earth," is used as a basis to work from[2]. From Mr R. Mallet's late investigations on the contraction of slag from an iron furnace[3], a probable coefficient of contraction for melted rock is deduced, viz. 0·0000217 for 1° Fahr.; and with this is obtained a value for $2\,ke$, or the thickness

[1] *Nature*, 1872, Vol. v. p. 479.

[2] *Edin. Trans.* 1862 ; and *Natural Philosophy*, p. 711.

[3] *Royal Soc. Trans.* 1873.

of the coating above defined. Sir W. Thomson's investigation proceeds upon the supposition, founded upon Bischoff's experiments upon the contraction of melted rocks in cooling, that, if the earth, or an outer coating of it, were once in a molten state, then, as soon as a crust began to form, it would break up and sink, and thus the whole would be reduced to the temperature of incipient solidification before it could be permanently crusted over. From the time of such incipient solidification it has gone on cooling, subject to the laws of cooling of a solid.

He then proves that upon this supposition the temperature would increase from the surface downwards, at first at a nearly uniform rate, but at a greater depth much more slowly, until at a certain point such a temperature would be arrived at, as would be about sufficient to induce fusion under the pressure existing at that depth. Now the rate at which the temperature first begins to increase is known to be about 1° Fahr. for 51 feet. Sir W. Thomson has determined, by observation on the rocks at Edinburgh, that their conductivity on an average is 400. With these data he proves that if, for the sake of illustration, the temperature at which the crust began to solidify be taken at 7000° Fahr., then the time since such solidification commenced will have been about one hundred millions of years, and that at about 100 miles below the surface the melting temperature would be reached.

Proceeding upon these assumptions, with the coefficient of contraction for rock above mentioned, the value of $2\,ke$ is calculated, or the thickness of the coating which all the elevations would form if they were levelled down, and it is found to come out less than 800 feet.

Still further, if instead of 7000° Fahr. 4000° is assumed to be the temperature for melting rock, which seems to be justified by Mr Mallet's experiments, then the value of $2ke$, or the thickness of the coating referred to, would be less than 150 feet. In the latter case the time since solidification commenced would be about thirty-three millions of years.

If we compare the values thus found upon two different suppositions respecting the temperature of melting rock (one of them being extravagantly large) with the value for the same measurement as determined by estimating the actually existing inequalities of the earth's surface, we cannot but be struck with the immense discrepancy between them, the latter being from 12 to 66 times as large as the former. The author is consequently led to doubt the necessity for accepting Sir W. Thomson's restrictions upon the manner in which the earth has come into its present state, especially since it seems now generally admitted that Bischoff's results concerning the contraction of melted rock cannot be relied upon. This was pointed out in 1868 by Mr David Forbes, and quite recently by Mr Mallet, who has determined the contraction in passing from a molten to a solid state to be scarcely 6 *per cent.*, instead of 25 *per cent.*, as stated by Bischoff. Probably, therefore, when we take into account the intermediate condition of viscosity, we need not assume the breaking up and sinking of a crust formed over a molten globe. This view is supported by what Mr Scrope tells us about a lava stream remaining liquid, and even more or less in motion in its central and lower portion for years[1]. Indeed, Sir W. Thomson is careful not to exclude as impossible " the case of a liquid globe gradually solidifying from without inwards, in consequence of heat conducted through the solid crust to a cold external medium."

If this has been what has happened, there may have been a much larger nucleus inclosed within the crust in early times than we have at present, and thus the corrugations formed would have been larger. And a great portion of that nucleus consisting of superheated rocks in a state of igneo-aqueous fusion, much of the water may have escaped in steam during the frequent volcanic outbursts of pristine ages, so that a large portion, at any rate, of the oceans now above the crust may have

[1] *Volcanos*, 2nd ed. p. 84.

been originally confined beneath it : and thus a much greater amount of contraction may have taken place than mere cooling would account for.

It is obvious that this reasoning will apply equally well to the case of a solid globe originally covered with a sufficiently deep layer of molten rock, which is the condition supposed by Sir W. Thomson to be the most probable, a view strongly supported by Dr Sterry Hunt[1], and more in consonance with the rigidity considered requisite to obviate the production of internal tides. But at the same time it is to be remarked, that a highly fluid original condition of the interior may have lasted long after mountains commenced to be formed, and yet its condition need not continue such at the present time.

February 2, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr F. A. PALEY gave a summary of a paper intended to shew that Thucydides must have been mistaken in describing what was really the city-wall of the Platæans, with its battlements and towers, as a temporary wall erected in three months by the besiegers. The paper contended that the Spartan army had got possession of and manned the city-wall, wishing to reduce the Platæans to the necessity of capitulating; and for this a political reason was given. Doubts were thrown on the account of a double wall and double moat, since the researches of modern travellers, which were quoted, did not bear out the statement, and no traces of either existed, though the ruins of the city-walls still remain in great part. It was shewn that ancient Greek cities had precisely such walls as Thucydides describes; and his veracity in the account was impugned, on the supposition

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[1] *American Journal of Science*, Vol. v. p. 264.

that he sacrificed strict truth for the purpose of writing a romantic and sensational story.

Mr J. B. PEARSON, of Emmanuel College, read a short paper on Eur. Phœn. 1115—1118, intended to establish its probable genuineness. He pointed out that the legend of Argus was an old and well-known one, and argued that the grammatical difficulties occurring in the passage were not insuperable. Admitting that the poet was desirous to introduce an elaborate and somewhat novel scene out of the legend of Thebes, he suggested that anything uncouth or extravagant in the passage might well be ascribed to poetic licence. Mr Pearson also stated that the authority of the MSS. and Scholiasts was unanimous in recognizing it, as is not always the case with passages intrinsically questionable; and that it was allowed by some, though not all, the best editors, especially Porson, who here dissents from the opinion of Valckenaer whom he generally follows.

February 16, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

(1) *On the geometrical representation of Cauchy's theorems of Root-limitation. By* Professor CAYLEY.

There is contained in Cauchy's Memoir "Calcul des Indices des Fonctions," *Jour. de l'Ec. Polyt.* t. XV. (1837) a fundamental theorem, which, though including a well-known theorem in regard to the imaginary roots of a numerical equation, seems itself to have been almost lost sight of. In the general theorem (say Cauchy's two-curve theorem) we have in a plane two curves $P = 0$, $Q = 0$, and the real intersections of these two curves, or say the "roots," are divided into two sets according as the Jacobian

$$d_x P \cdot d_y Q - d_x Q \cdot d_y P$$

is positive or negative; say these are the Jacobian-positive and the Jacobian-negative roots, and the question is to determine for the roots within a given contour or circuit, the difference of the numbers of the roots belonging to the two sets respectively.

In the particular theorem (say Cauchy's rhizic theorem) P and Q are the real part and the coefficient of i in the imaginary part of a function of $x + iy$ with in general imaginary coefficients (or what is the same thing, we have

$$P + iQ = f(x + iy) + i\phi(x + iy),$$

where f, ϕ are real functions of $x + iy$): the roots of necessity are of the same class: and the question is to determine the number of roots within a given circuit.

In each case the required number is theoretically given by the same rule, viz. considering the fraction $\dfrac{P}{Q}$ it is the excess of the number of times that the fraction changes from $+$ to $-$ over the number of times that it changes from $-$ to $+$, as the point (x, y) travels round the circuit, attending only to the changes which take place on a passage through a point for which P is $= 0$.
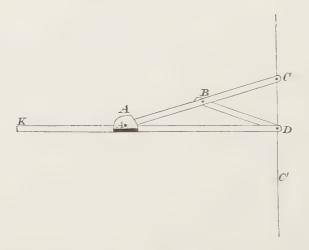
In the case where the circuit is a polygon, and most easily when it is a rectangle, the sides of which are parallel to the two axes respectively, the excess in question can be actually determined by means of an application of Sturm's theorem successively to each side of the polygon, or rectangle.

In the present memoir I reproduce the whole theory, presenting it under a completely geometrical form, viz. I establish between the two sets of roots the distinction of *right-* and *left-handed:* and (availing myself of a notion due to Prof. Sylvester) I give a geometrical form to the theoretic rule, making it depend on the "intercalation" of the intersections of the two curves with the circuit: I also complete the Sturmian process in regard to the sides of the rectangle: the memoir

contains further researches in regard to the curves in the case
of the particular theorem, or say, as to the rhezic curves
$P = 0,\ Q = 0$.

A communication was also read by Professor Cayley
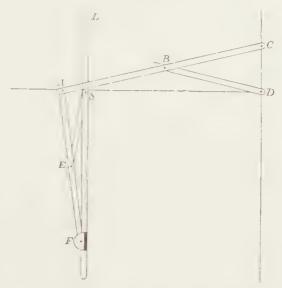
(2)   *On Peaucillier's Parallel Motion.*

(3)   *On some models of Peaucillier's and other Parallel
Motions.   By* Mr ELLIS.

DK is a fixed guide upon which A slides. ABC is a rod
moveable round a pin at A and bisected in B. BD is a rod
whose length is $\frac{1}{2}AC$ and moveable round pins at B and D.
It is manifest that C will trace out a straight line CDC' per-
pendicular to KD.   Supposing $CAD\ (= C'AD)$ when greatest
to equal $45°$, the space described by C to space described by
$A :: 2 : \sqrt{2} - 1$, or as $5 : 1$ nearly.   This method may therefore
often be employed with advantage to reduce friction instead of
employing a guide for C.   The friction may be again reduced
almost to any extent by the following arrangement.

The point A instead of sliding is attached by a pin to the

extremity of the rod AF. FSL is a fixed guide at right angles to AD upon which the extremity F of AF slides.

E is the middle point of AF, and ES is a rod equal in length to $\frac{1}{2}AF$, and moveable round pins at ES.
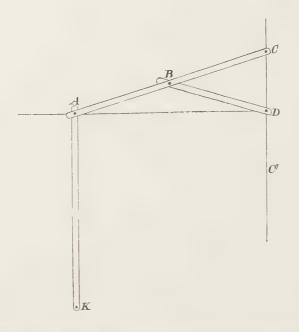
The motion of F may be made as small as we please by increasing the length of AF.

Hence we may approximate as closely as we like to a case of linkwork where the friction is entirely reduced to that round pivots.

For example, if C were attached to a piston-rod whose travel was $10''$, the travel of A would be $2''$, and the travel of F would be less than $\frac{1}{25}''$, if the rod AF equalled AC.

We might moreover do away with the sliding of F by making it the extremity of a third rod, and so on.

Looking back at fig. I. we see that C would describe a straight line very nearly if KD instead of a straight line were the arc of a very large circle; and this reasoning may originally have suggested to Watt his parallel motion.

Thus if A instead of sliding on a guide be attached by a pin to the extremity of a long link AK, whereof the other extremity K is moveable round a fixed pin k, A will describe a small portion of the arc of a large circle, and therefore move approximately in a straight line. This is in fact a case of Watt's parallel motion. The fixed point k might have been above the line AD.
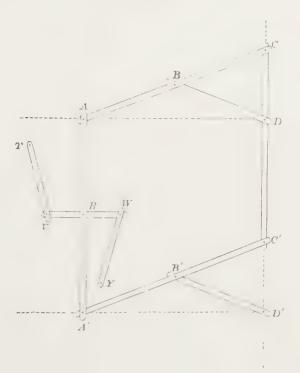
It is to be observed that C will describe an approximate straight line for a considerable space, if A for a short space describes an approximate straight line.

Hence we have only to make A move in a straight line for a short space by any means we can: for instance, by means of Watt's parallel motion.

The above remarks will explain the rationale of the following model in linkwork in the late Prof. Willis' collection.

$ACC'A'$ is a parallelogram hinged at A, C, C', A'; B bisects
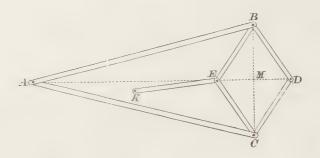
AC and B', $A'C''$; BD, $B'D'$, each equal in length to $\frac{1}{2}AC$, are rods moveable about the fixed pins DD'.

TV, VW, WY are equal rods connected by pins at V and W. VW is attached to AA' by a pin through its middle point R. T and Y are pins fixed in the framework. R therefore by a Watt's motion describes an approximate straight line perpendicular to AA'. Now AA' is parallel to BB', and therefore to DD', which is a fixed straight line. Hence AA' moves parallel to itself.

$\therefore A$, R, A' describe parts of straight lines perpendicular to AA'.

Hence by what precedes CC' moves in a fixed straight line.

Peaucillier's motion.

$BECD$ is a rhombus formed of equal rods jointed by pins at B, D, C, E. It is jointed by pins at B and C to two equal rods AB, AC, which are moveable about a fixed pin at A.

It is manifest that the straight line AE makes equal angles with EB and EC, and will therefore if produced pass through D.

It is also manifest that

$$AD \times AE = AM^2 - EM^2 = AC^2 - EC^2 = \text{a constant.}$$

Hence whatever curve is traced out by the point E, say $\rho = F(\theta)$, the curve traced out by D will be $\dfrac{1}{\rho'} = F(\theta)$, when $\rho = AE$ and $\rho' = AD$. Hence, if E is made to describe a circle passing through A, D will describe a straight line. E is made to describe this circle by means of a bridle-rod $KE (= KA)$ moveable round a fixed pin at K.

March 2, 1874.

The President (Professor Babington) in the Chair.

On the Relation of Geometrical Optics to other parts of Mathematics and Physics. By Prof. Clerk Maxwell.

The study of geometrical optics may be made more interesting to the mathematician by treating the relation between

the object and the image by the methods used in the geometry of homographic figures. The whole theory of images formed by simple or compound instruments when aberration is not considered is thus reduced to simple proportion, and this is found very convenient in the practical work of arranging lenses for an experiment, in order to produce a given effect.

As a preparation for physical optics the same elementary problems may be treated by Hamilton's method of the Characteristic Function. This function expresses, in terms of the coordinates of two points, the time taken by light in travelling from the one to the other, or more accurately the distance through which light would travel in a vacuum during this time, which we may call the *reduced path* of the light between the two points. The relation between this reduced path and the quantity which occurs in Cotes' celebrated but little known theorem, is called by Dr Smith the " apparent distance." The relations between the " apparent distance " and the positions of the foci conjugate to the two points, the principal foci and the principal focal lengths, were explained; and the general form of the characteristic function for a narrow pencil in the plane of xr was shewn to be

$$V = V_0 + \mu_1 r_1 + \mu_2 r_2$$
$$+ \frac{1}{2} \frac{\mu_1 (r_2 - \alpha_2) x_1{}^2 + \mu_2 (r_1 - \alpha_1) x_2{}^2 - (f_1 \mu_2 + f_2 \mu_1) x_1 x_2}{(r_1 - \alpha_1)(r_2 - \alpha_2) - f_1 f_2} + \&c.,$$

where r_1, r_2 are measured from the instrument in opposite directions along the axis of the pencil in the media μ_1, μ_2, respectively, and x_1, x_2 are perpendicular to the axis.

α_1, α_2 are the values of r_1, r_2, for the principal foci, and f_1, f_2, the principal focal lengths, and $f_1 \mu_2 = f_2 \mu_1$.

If
$$\frac{r_1 - \alpha_1}{f_1} = \frac{f_2}{r_2 - \alpha_2} = \frac{x_1}{x_2},$$

the last term of V assumes the form $\dfrac{0}{0}$, and an infinite number

of possible paths exist between the points (x_1, r_1), and (x_2, r_2), which are therefore conjugate foci.

Differentiating V with respect to x_1 and x_2 we obtain

$$\frac{d^2V}{dx_1 dx_2} = \frac{1}{D} = -\frac{1}{2}\frac{f_1\mu_2 + f_2\mu_1}{(r_1 - \alpha_1)(r_2 - \alpha_2) - f_1 f_2} + \&c.,$$

D is the quantity in Cotes' Theorems which Dr Smith calls the Apparent Distance, or the distance at which the object must be placed that it may subtend the same angle as when viewed through the instrument.

We have also

$$f_1\mu_2 \frac{dD}{dr_1} = \alpha_2 - r_2, \quad f_2\mu_1 \frac{dD}{dr_2} = \alpha_1 - r_1.$$

March 16, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Communications were made to the Society:

(1) *On the use of the term Endothelium.*
By Dr MICHAEL FOSTER.

In this paper it was shewn that the term "endothelium" has been recently introduced into histology: and the use of it has rapidly become common if not general. The speedy acceptance of a new term may in many cases, though not all, be taken as an indication that something of the kind was wanted: and the already frequent use of "endothelium," both by Continental and English Histologists, would seem to shew the need of some other phrase besides "epithelium." Nevertheless there are cogent reasons why the new term should not be allowed to take farther root.

In the first place, its etymology is of the most grotesque kind. This is of course an objection of secondary value, but has still some weight. When a term has once come into daily

use with a well-defined meaning attached to it, it does not much matter what its etymology is, or how it is spelt, except on historical grounds. Many terms become so altered in their meanings, before they finally acquire a permanent application, that the chief interest in their etymology is confined to the light it throws on the ideas of the man who first introduced them. This is the chief reason why new terms should be etymologically correct, in order that future inquirers may read back through them into the minds of earlier observers. When a word is etymologically pure nonsense, this is apt to become impossible. Such is the case with "endothelium."

It appears to have been first introduced by His to designate the kind of epithelium ("unächte Epithelien") which is found lining the vascular, lymphatic, and serous cavities of the body, in contradistinction to the real epithelium of mucous membranes (see *Die Häute, &c. &c. Akad. Programm.* Basel, 1865). "Sei es, dass man sie als *unächte Epithelien* den *ächten* gegenüber stellt, sei es dass man sie Endothelien nennt, um mit dem Wort ihre Bezeichnung zu den innern Körperflächen auszudrücken."

Endothelium is here contrasted with epithelium, so that the latter may be considered as the "thelium" of free surfaces (whether invaginated or not), and the former as the thelium of internal closed spaces, "thelium" being apparently taken to mean "a layer," or "layers of cells."

Now what is the derivation of "epithelium"? Dr Sharpey gives the following account : he says, in a letter, "epithelium," or rather "epithelida," and especially "epithelia" (1st decl.), was first introduced by F. Ruysch. In describing a preparation of the face of a child finely injected, he refers to the cuticle over the red part of the lip (prolabium), and says, "I cannot call this 'epidermis,' seeing that the subjacent tissue is not skin, but a different substance covered with sensitive papillæ, which are finely injected red." He then goes on to say that as

the cuticle lies on papillæ, he will call it *epithelida* or *epithelia*, from ἐπὶ and θηλή "papillæ," or "mammilla," and he adds that for the same reason he calls the inside coating of the cheeks by the same name. (The original may be found, F. Ruysch, *Thesaurus Anatomicus*, III. No. xxiii. p. 16, " Nulla subest, &c. &c. papillarum"): and again VI. No. cxv. p. 49, he says..."Anterior pars prolabii anterioris—epitheliâ adhuc est obducta...".

From this it is evident that " epithelia " (changed in course of time into epithelium, just as platina becomes platinum) means 'that which covers or is upon a papilla,' and consequently " endothelium " means that which is inside a papilla. The extension of the phrase epithelium to the cellular covering of such parts of the corium as are destitute of papillæ may be easily allowed, but it seems a daring violation of propriety to apply the phrase " within the papilla " to the cells coating surfaces of which one great characteristic is that they are devoid of papillæ! There seems to be something attractive about "thelium" that tempts writers to make use of it. Already "endothelium" has given rise to " ectothelium," and probably " thelium " will soon become a kind of histological maid-of-all-work, with as many prefixes as there are kinds of cells.

In the second place, there are objections to the use of the term endothelium not etymological in their nature. Without considering the peculiar views of His on the connective tissues of the body, it still seems desirable to have some distinctive term to denote the epithelium which is formed out of the elements of the middle of the three layers of the germ (the *mesoblast* of Mr Huxley and myself), the word epithelium being reserved for the nether layer (or *hypoblast*). If so the word endothelium cannot be employed with this meaning, for it would then include structures still called epithelium, and differing in no essential characters from the epithelium derived directly from the hypoblast.

The cells lining the Wolffian duct and its derivative the ureter, with their branches, would then come under the heading endothelium. Whatever be the first formation of the Wolffian duct, whether by the central solution of a solid ridge, or by an infolding of the lining of the pleuro-peritoneal cavity, it is lined by cells which are clearly mesoblastic in origin. not hypoblastic, nor, as was once suggested, epiblastic.

The case of Müller's duct is still more clear. This undoubtedly arises by an infolding of the lining of the pleuro-peritoneal cavity. Its epithelium is distinctly mesoblastic in origin. The germinal epithelium which gives rise to the ovaries is also essentially mesoblastic.

If the word endothelium, then, be taken to denote an epithelium derived from the mesoblast, it must be extended to include the epithelium of the Wolffian and Müllerian ducts, and of the parts which are formed ultimately out of these structures. But if these be included, the phrase loses all its practical utility. If they are excluded, all the little meaning it ever had, vanishes.

It may be urged that we need a word to denote the epithelium which is found in the vascular and lymphatic spaces. There does not however appear to be sufficient reason why the same term should be applied to the whole of this epithelium. As we have seen, its common mesoblastic origin will not justify this. From a structural point of view, three distinct varieties may be recognized in it, viz. the spindle-shaped cells of the blood-vessels and larger lymphatic vessels, the sinuous cells of the commencing lymphatics, and the polygonal cells of the large serous cavities. The fact that the epithelium of the peritoneum is continuous with that of the lymphatics, affords no argument at all for classing them together. We find continuity everywhere. The epidermis is continuous with the alimentary epithelium, and with the urinary and generative epithelium; and the generative epithelium is in turn continuous with the

peritoneal epithelium. In short, there is no reason why the cells spoken of as forming endothelium should have a common title, distinct from the general term epithelium.

The introduction of the new term is really a step backwards from, instead of an advance beyond, the old classification given in Quain's *Elements of Anatomy*, where epithelium is divided either physiologically into epidermic, mucous, glandular, vascular, serous, &c., &c., or structurally, into columnar, spheroidal, ciliated, tesselated, squamous, &c., &c. Some such nomenclature as this satisfies all requirements, either morphological or physiological. The chief morphological importance, as far as our knowledge goes, attaches itself to the question, from which of the three primary layers, epiblast, hypoblast, or mesoblast, any given epithelium is derived; for physiological purposes, all we need is some system of phrases which shall clearly indicate the individual characters and the arrangement of any group of cells; and these requirements are met by the phrases enumerated above. We do perhaps want easy terms denoting whether the epithelium in any spot consists of several layers, or of one pronounced layer only; *monoderic* may be proposed for the latter, *polyderic* for the former case. Epithelium itself would only mean cells lining a cavity or coating a free surface.

(2) *On some Problems on the Physiology of Nutrition, and the methods of solving them. By* Dr MICHAEL FOSTER.

(3) *On an Experiment of Galileo. By* Mr SEDLEY TAYLOR.

Mr Sedley Taylor drew attention to an observation made by Galileo, and described by him in the first of his *Dialoghi delle nuove scienze*[1]. Galileo says that while scraping a brass

[1] Vol. XIII. pp. 104, 105, of the Florentine edition of Galileo's complete works.

plate with a chisel in order to remove some spots, he noticed that the passage of the instrument across the plate occasionally produced a powerful and distinct musical note, and that, when this happened, a long row of fine equidistant striations was left on its surface. These marks were closer together when the sound was acute than when it was grave. Having produced by the above means two notes which differed by an exact Fifth, Galileo measured the distance between their respective striæ, and found that *three* of those corresponding to the upper note occupied precisely the same space as *two* corresponding to the lower. He hence inferred that the numbers of vibrations executed in the same time by any two notes forming this interval are in the ratio of 3 : 2 ; a conclusion which had previously been only conjectured from results obtained by the monochord. Galileo remarks further that the same principle applies to the case of *any* interval.

Mr Taylor exhibited a brass plate with rows of striæ upon it obtained by screwing the plate into a lathe, and, while it was rotating, holding the edge of a chisel against it in such a way as to produce a musical sound. The markings were in some cases extremely fine and regular.

April 27, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Communications were made to the Society :

(1) *On the use of the "Ligamentum Teres" of the hip-joint. By* Mr SAVORY, F.R.S.

This paper discussed the proper use of the "ligamentum teres," which, though variously stated, has not, it was maintained, been correctly given. The statement that the ligament is vertical and tight, when the person is erect, has been chal-

lenged : but the author was satisfied of its accuracy. It could
be demonstrated by removing the bottom of the acetabulum
with the trephine. The ligament is moderately tight when a
person stands evenly upon both legs. It is tighter when the
femur is slightly flexed, as it usually is. But when resting
upon one leg, inasmuch as the pelvis is then raised on that
side, which of course affects the ligament in the same way as
adduction of the femur would do, then the ligament becomes
extremely tense. In other words, it becomes tightest when
the hip-joint has to sustain the greatest weight. When there-
fore the pelvis is borne down upon the femur, or when the
femur is forced upwards—that is when the pressure would
be greatest between the upper part of the acetabulum and the
opposite surface of the head of the femur—it is put directly
on the stretch. More precisely, its great purpose is to prevent
undue pressure between the upper portion of the acetabulum,
just within the margin, and the corresponding part of the
head of the femur. But for this ligament such undue pressure
must inevitably occur. Suppose the *ligamentum teres* absent
and the person standing upright, owing to the obliquity of the
acetabulum and the head of the femur, pressure between the
two could not be equally, or nearly equally. diffused over their
opposing surfaces, but it would be concentrated on a spot in
the upper part of the socket through which a line drawn down
the body, through the joint into the leg, would pass. When
the thigh is straight, when the femur is in a line with the
body, as when one stands upright, then is the ligamentum
teres in the same line too, and consequently any force which
drives the femur and pelvis together must tell at once upon
the ligament, and be directly checked by it. Owing therefore
to the shape and obliquity of the hip-joint, and the weight of
the body, the ligamentum teres is necessary to prevent con-
centration of pressure at a particular point above it. The
line through which the weight or force acts between the upper

part of the acetabulum and the opposed surface of the head of the femur, forms, with the line of weight of force which passes through the ligamentum teres, an obtuse angle : and the resultant of these forces is in a line which passes through the long axis of the head of the femur. When the person is erect, the body partly hangs upon the ligamentum teres. This, he submitted, is the prime function of the ligamentum teres. Other purposes he did not deny, but would maintain that they only occasionally come into play, and are altogether subordinate to this one, which is especially called into action whenever the weight of the body is thrown upon one leg. He supported his view by reference to comparative anatomy, remarking that it is present when the acetabulum looks outward, and the head of the femur is inclined inward : in other words, when the hip-joint is placed obliquely, so that there would be otherwise undue pressure at a particular part ; and that it is absent in those animals in whom, although it is an instrument of regression, the posterior extremity does but little in supporting the weight of the body : e.g. seals, and the ourang-outang.

In a discussion which followed, Prof. Humphry disputed, and Mr Savory still maintained, the tension of the ligament referred to in the paper.

(2) *On a Clepsydra.* By Mr ELLIS.

(3) *A Model shewing the mechanical arrangement of the Joints in the Limb of a Lobster.* By Mr ELLIS.

The following communication was made to the Society:

On the Bearing of the Distribution of the Portio Dura upon the Morphology of the Skull. By T. H. HUXLEY, Sec. R.S.

In the first place, the distribution of the seventh nerve or *portio dura* in Man was compared with that of the same nerve in the *amphibia;* and it was shewn that, while the proper facial nerve, with the *chorda tympani*, corresponds in all essential respects with the posterior division of the seventh nerve in the Frog and other amphibia, the *nervus petrosus superficialis major* or vidian nerve, with its palatine branches and the nerve of Cotunnius, answers to the anterior division of the seventh, or so-called " palatine " nerve of the Frog. A branch which, in the *Urodela*, connects the *portio dura* with the Gasserian ganglion, appears to be the homologue of the *nervus petrosus superficialis minor*. The tympano-Eustachian passage, in both Man and the Frog, is included between the two main divisions of the *portio dura*.—The distribution of the seventh nerve in the Ray was next described. Its two divisions were shewn to have the same relation to the spiracle as they have to the tympano-Eustachian passage in the higher vertebrata. The anterior division, however, differed from that of the Frog and that of Man, in possessing no branch comparable to the nerve of Cotunnius. The place of this nerve appears to be taken by a large 'palato-nasal' branch of the fifth (as Bonadorff has already suggested), and it was suggested that the Cotunnian branches of the palatine nerves in the Frog and in Man really belong to the Trigeminal. The distribution of the *portio dura* was then

compared with that of the glossopharyngeal and that of the branchial branches of the vagus, and the conclusion was drawn, that the *portio dura* is the nerve of the mandibulo-hyoid cleft (commonly called the first visceral cleft), and is distributed to the (morphologically) anterior and posterior walls of that cleft. As a corollary from this conclusion, it followed that the pterygoid arcade does not represent an independent visceral arch, but is a dependence of the mandibular arch, as Gegenbaur has already argued upon other grounds. It was further shewn that the distribution of the second and third divisions of the fifth nerve is such as accords with the view that they represent the posterior division of the nerve of the trabeculo-mandibular cleft. The anterior division of that nerve was sought in the palato-nasal branch of the trigeminal—while the first division of the latter nerve appears to be the nerve of the (morphologically) anterior face of the trabecula. The sixth, third, and fourth nerves were regarded as special branches of the nerves of the mandibulo-hyoid, and trabeculo-mandibular clefts respectively, developed in relation with the special muscles of the eye. The author finally endeavoured to shew that the results thus obtained by the thorough investigation and comparison of the distribution of the cranial nerves were in entire accordance with those obtained by the study of development, and that the apparent anomalies in the distribution of the fifth and of the seventh nerves in the higher *vertebrata* are easily explained by the metamorphoses of the trabecular and mandibular and hyoidean arches in these animals.

Professor HUMPHRY expressed his thanks and the thanks of those present to Prof. Huxley for the careful and lucid account which he had given of a difficult piece of anatomy, and for the interesting and morphological inferences which he had deduced from them, and also for the illustration he had given of the fact that the dullest, most troublesome anatomical details may be brightened, and so rendered easy by an insight into their true

meaning. This was really the way to study anatomy, viz. to regard the various facts in connection with other facts, and so as the bases of scientific deductions. Prof. Humphry was glad to hear the nerves thus made the exponent of cranial morphology, for he had attempted the same thing many years ago in a paper read at the British Association at Leeds, when he endeavoured to shew that the fore limb was not, as supposed by Prof. Owen, an appendage to the skull, but formed independently from it. He then shewed, from a consideration of the distribution of the cranial nerves, that the hyoid and not the scapula is the visceral arch of the occipital, and that the mandibular, the pterygo-maxillary and the ethmo-vomerine arches are the respective visceral arches of the post-sphenoidal, the pre-sphenoidal, and the ethmoidal parts of the skull. This view he believed to be in the main correct. The nerves respectively supplied to them are the ninth and the three divisions of the fifth. Each of the latter is very closely confined to its particular visceral arch, sending a special nerve to each bone of its arch, or nearly so, whereas the seventh pair of nerves is more promiscuous in its distribution, being supplied to muscles disposed upon all the four visceral arches, and having connecting links with the spinal nerves of those arches. It was to the orderly disposition of these connective links in relation to the visceral arches that Professor Huxley had now called their attention. Professor Humphry remarked that the communication between different nerves, which is a means of establishing the harmonious action of the several muscles supplied by them, was effected in three ways. *First*, by junction of their terminal branches. This is most common in the lower animals. *Secondly*, by plexuses near their origin from the brain and spinal cord, which are found, to some extent, in the lower animals, but which are more numerous in the higher animals. *Thirdly*, by means of ganglia. This last, which may be regarded as the most perfect method, is almost confined to the higher animals. Accordingly the communicating

branches between the seventh and fifth, which formed the subject of the author's paper, pass to Meckel's ganglion, the otic ganglion, and the submaxillary ganglion in Mammals; whereas in Batrachians they do not pass to these ganglions, but their junctions are effected among the terminal ramifications of the nerves. He could not agree with Prof. Huxley that the fore part of the skull was not, like the hinder part, composed of vertebral elements. It was transversely segmented after the manner of the rest of the skeleton, and these segments are vertebræ, whether the notochord exists at the part or not; and whether the segmentation takes place early or not, that is, in the cartilage or in the osseous nuclei developed in the cartilage, makes little difference. Sooner or later, in the higher animals at any rate, the segmentation occurs. The foremost elements derived from the trabeculæ had been designated as ribs by Prof. Huxley; and if they are so, they are components of those segments, of which the vertebræ form the mesial elements. He could not quite accept the view of the homologies of the mandibular arch which had been given; but time failed to discuss these questions more fully. He concluded by again thanking Professor Huxley for this interesting communication.

May 25, 1874.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr PEARSON read a paper on some meridian observations of the Sun taken by him with a prism-circle and an artificial horizon, at Taormina in Sicily, on April 1st last. They were taken with the view of determining the latitude of the place. The watch was set to Greenwich time, but about 8m. 9s. slow.

The observations were taken before and after noon, and, reduced, were as follows:

	h.	m.	s.	Altitude		
(1)	10	46	0	56°	41'	29''.
(2)	10	49	30	56°	43'	29''.
(3)	10	53	0	56°	44'	59''.
(4)	10	58	50	56°	44'	19''.
(5)	11	1	0	56°	43'	9''.
(6)	11	3	40	56°	41'	29''.
(7)	11	12	40	56°	30'	19''.

A comparative examination of these suggested 10h. 54m. 35s. as the probable time of apparent noon. (The method for ascertaining the time of noon given by Godfray, Ast. art. 150, was not available at the time the observations were worked out: by it, the times of app. noon on the mean of two separate observations are as follows: (1) and (2) 10h. 54m. 0s., (3) and (4) 10h. 54m. 40s., (4) and (5) 10h. 54m. 25s., (5) and (6) 10h. 54m. 15s.,...average 10h. 54m. 20s.)

The method employed to find the latitude is that given in Raper's "Navigation." Tables are given containing a given series of numbers varying for all latitudes and declinations. The number in this particular case (Lat. 37°. 50'. 50'' N., Sun's Decl. 4°. 34'. 6'' N.) is ·458.

This is added to the sin. sq. of the time elapsing between the time of observation and that of apparent noon: the result is the log. of the sin. of the difference between the altitude of the sun at the time of observation and its meridian altitude.

Employing this method we get these results: For observation (1) Lat. 37°. 49'. 0''; for (2) 49'. 19''; (3) 48'. 56''; (4) 49'. 0''; (5) 49'. 0''; (6) 48'. 53''; (7) 48'. 43''; average 37°. 48'. 58''. 7.

The methods given in Norie's "Navigation," and in Godfray's Ast., art. 149, produce very nearly the same results; e.g. obs.

(6) worked out (a) on Norie's method gives Lat. 37 . 49'. 10".
(b) on Godfray's method gives Lat. 37°. 48'. 54".

The hour-angle of the Sun. by obs. (7), on the theory
(1) that the lat. of the chart is correct. and that the instru-
ments were in adjustment, is 16m. 45s.—an error of 1m. 20s.;
(2) that there was an error of about 2' in one of the two, is
17m. 57s.—an error of 8s., which tends to prove the existence
of some such error.

The present Admiralty chart, issued as newly corrected,
1873, gives 37 . 50'. 50" as the lat. of the spot where the obser-
vations were taken. At first sight, this would seem to prove an
error of nearly 2', either in the instrument or the artificial
horizon, as levelled at the time.

But (1) Admiral Smyth, in his survey of Sicily, carried out
in 1813—15, places Taormina in 37 . 48'. 40". (2) He states
in his book on the subject that he was remarkably well supplied
with sextants and other surveying instruments. (3) The long.
and lat. of the principal points on the coast given by him often
agree with those now given in the charts. (4) His estimate of
the height of Etna, obtained by triangulation from a base on the
sea, viz. 10,874 ft., is *very* nearly accurate, that recently obtained
by levelling being 10,840 ft. (5) The lat of Taormina, as now
given in the charts, agrees exactly with, and may possibly be
borrowed from, that given by Baron von Waltershausen, in his
survey of Mt. Etna and its environs, executed from 1840 to
1850, and may therefore be not perfectly accurate.

On these grounds it was argued that the latitude of Taor-
mina, as given by Adm Smyth, and (approximately) by this set
of observations, may perhaps be more nearly accurate than that
given in the present charts: at any rate they shew that it is
perfectly feasible for a person, with simple instruments and
merely arithmetical processes, to determine his latitude, in any
part of the globe, with reasonable accuracy.

Dr CAMPION said, that from practical experience, he was

aware of the uncertainty attending any set of observations made by a single individual.

Prof. CAYLEY suggested a diagram, similar to those given in meteorological reports, indicating, by a curved line and dots, the altitude of the Sun at different times: from which the meridian altitude and its time might be approximately inferred.

The Secretary then read a paper

On the Temperature of the Earth in times anterior to the Eocene period.   By Mr RÖHRS.

Mr RÖHRS stated that geological evidence seemed to point to a warm and equable climate over a great part of the earth in præcocene days.   He thought it probable that this high temperature was due to the internal heat of the earth, and that the amount of heat radiated by the sun and received by the earth may have been less than it is now—the solar atmosphere obstructing radiation more than at present, although the energy and mean temperature of the sun were greater in early times. He referred the first great glacial period to a time when the internal heat of the earth was diminishing, and the solar radiation had not reached its present amount.

Mr SOLLAS said that there was clear evidence of ice-action at various epochs long anterior to the Eocene period, so that Mr Röhrs' theory of a long period of uninterrupted high temperature was geologically untenable.

October 19, 1874.

PROFESSOR CAYLEY in the Chair.

On some Ice-hummocks in the Gorner Glacier.
By Mr TROTTER.

The speaker described some remarkable water-holes asso-
ciated with hummocks of ice on parts of the Gorner Glacier
which he had observed in 1863 and in 1874. The water-holes
were oval in shape with their longer axes parallel and pointing
east and west, the hummocks were on the south side of the
water-holes. The direction was independent of that of the
veined structure, and the whole was obviously a meridian
phenomenon, but several points as to the origin of the hum-
mocks and the shape of the water-holes were very obscure.

An examination of them earlier in the season would
probably throw some light on their origin.

ANNUAL GENERAL MEETING, *October* 26, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following were elected officers for the ensuing year :

President.

Professor CHARLES C. BABINGTON, F.R.S.

Vice-Presidents.

Professor MAXWELL.
Professor MILLER.
Mr MUNRO.

Treasurer.

Dr CAMPION.

Secretaries.

Mr J. W. CLARK.
Mr TROTTER.
Mr PEARSON.

New Members of Council.

Professor LIVEING.
Mr JACKSON.
Mr GLAISHER.

On a nearly complete Skeleton of the Bos Primigenius found in Burwell Fen. By Mr J. W. CLARK.

Mr J. W. CLARK exhibited and made some remarks upon a skeleton of the great extinct Ox (*Bos primigenius*). The bones had been found together in Burwell Fen early in the spring of 1874; and there could be no doubt that they belonged to the same animal. The parts wanting are the right hind-leg, one lumbar vertebra, a few terminal vertebræ of the tail, and a few bones of the carpus, tarsus and toes. The skeleton, after the bones had been properly treated with gelatine had been mounted and placed in the Museum of Comparative Anatomy. It is the first skeleton found in England in a sufficiently perfect state to allow of its being articulated. Mr Clark briefly recapitulated the history of the species, shewing, from the passages out of chronicles and other contemporary records collected by Mr Boyd Dawkins, that it had subsisted in a living state on the continent of Europe down to a much later date than had been supposed previous to his researches.

November 2, 1874.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On some further Observations with a Prism-circle. By Mr PEARSON.

This paper was intended as a sequel to one read on May 25th, the two being intended to establish by practical examples the facility with which a traveller may establish his latitude and longitude in any part of the globe.

The following observations were taken Sept. 5th a.m. in Lat. 52° 12' 10" N.; Long. 30s. (7' 30") E. The time given is local mean time.

		Sun's Alt.	App. dist. of Sun from Moon.	
h. m. s.			h. m. s.	
7 42 27		21° 25' 40"	7 47 36	62° 54' 40"
7 53 44		23° 6' 0"	7 51 1	62° 53' 40"

The observation of the Sun at 7h. 42m. 27s. giving the local time obtained from the Philosophical Society's clock accurately within 7s., the levelling of the art. horizon, and so the altitude of the sun may be taken as nearly correct.

The above observations reduced give

At 7h. 48m. 19s. (L. M. T.) alt. of Sun's centre 21' 59' 42".

True app. dist. of centres of Sun and Moon 63° 25' 54".

The Moon being in 28° N. Dec. and having just passed the meridian (at 7h. 44m. 11s.), the altitude was obtained by calculating the Reduction to the meridian, instead of by the more difficult method of observation, the change in alt. in 4m. 8s. being only about 44'. This gave the true altitude of the Moon's centre 65° 47' 50". Of course the altitude of the Moon could only have been approximately ascertained in this way, had not the longitude of the place of observation been accurately known.

These data were worked out on four methods: 1st, on the plan given in Woodhouse's *Astronomy*, which is believed to be based more or less nearly on the formula originally devised by the Chevalier Borda; 2nd, on that given in the Introduction to Shortrede's Logarithmic Tables, mainly identical with the first method; 3rd and 4th, on the two methods given in Arts. 700, 701 of Raper's *Navigation*.

No. 1 gives the longitude of the place of observation 1s. W. of Greenwich; error 31s.

No. 2 gives it 3s. E. of Greenwich; error 27s.

No. 3 makes it 12s. E. of Greenwich; error 18s.

No. 4, 3s. W. of Greenwich ; error 33s.

A second set of observations gave at 8h. 5m. 13s. local time, true alt. of Moon 65° 29' 20''; true alt. of Sun 24° 10' 0''; true app. dist. of their centres (mean of three observations) 63° 20' 27''.

These data, when computed on the 1st method mentioned above, give the long. of the place 1m. 35s. E.; error 1m. 5s.: on the 4th method 1m. 29s. E.; error 59s. It was thought unnecessary to make the calculations again on the 2nd method, because its form is nearly the same as that of the first; on the 3rd, because its result in the previous case differed considerably from the other three.

The bar. and ther. were not taken into account, as neither of them were far from the point at which they do not affect the refraction. The index error of the instrument was too small to be ascertained by any one but a very good observer. The inaccuracy of the second set of observations might be due to a haze coming on at the time, and to the increasing brightness of the Sun.

Several extracts were also given from the voyages of Cook and Krusenstern, bearing on the accuracy with which it was found practicable to use this method of fixing the long. at the end of the last and the beginning of the present century. For example, the long. of Santa Cruz (Teneriffe), where Cook is described (Voyage, Vol. I.) as having met Borda in August, 1776, was given by the latter as 18° 35' 30'' W. of Paris. Cook making it by his timepiece 16° 31' 0'', by two sets of lunars 16° 30' 45'' W. of Greenwich. The true long. as given by the English Admiralty Chart (1873) being 16° 14' 56'' W. of Greenwich, or 18° 35' 6'' W. of Paris.

Nov. 16, 1874.

The President (Professor Babington) in the Chair.

Linear partial Differential Equations, and their Germ-integrals. *By* S. Earnshaw.

This paper will be found printed at length in the " Transactions of the Society."

[*Abstract.*]

Long before it was discovered that

$$u = \frac{A}{\sqrt{x}} \cdot \epsilon^{2\sqrt{xy}} + \frac{B}{\sqrt{x}} \cdot \epsilon^{-2\sqrt{xy}}$$

is an integral of the equation $\dfrac{d^2 u}{dx\,dy} = u$, it had been known that every linear partial differential equation with constant coefficients, whatever be the number of its variables, is susceptible of an integral of the form

$$u = C\epsilon^{mx+ny+\cdots}.$$

It thus appeared that the above equation admits of two integrals of essentially different types. The same was found to be the case with the equation $\dfrac{d^2 u}{dx^2} = \dfrac{du}{dy}$; of which both

$$u = C\epsilon^{mx+m^2 y}$$

and

$$u = \frac{A}{\sqrt{y}} \cdot \epsilon^{-\frac{x^2}{4y}}$$

are found to be integrals; and they are also of essentially different types as integrals. This discrepancy of types created in me a desire to ascertain the significance and true origin of each, and their mutual dependence if any existed.

I have worked out the case of two independent variables, but the method I have adopted is applicable to an equation of any number of independents. It is shewn that in every integral certain constants, called *germs*, exist, or can be arbitrarily introduced into it if they do not already exist there; and by means of these a *germ-integral* can be found; and from this a series of *sub-integrals*; and the sum of these is the general integral of the proposed equation.

This method depends for its success on the circumstance that the differential equation from which the sub-integrals are obtained contains fewer independent variables than the proposed equation. Hence when the equation to be integrated is the general differential equation of the second order of two independent variables and constant coefficients, the sub-integrals are to be found from a differential equation of the second order of one independent variable; and by its integration that of the general equation of the second order is accomplished.

The same method is shewn to be successful in the integration of certain other equations where the coefficients are not constants, but functions of the independent variables.

Nov. 30, 1874.

PROFESSOR HUMPHRY in the Chair.

(1) *On Lopsided Generation, or Right-handedness.* *By* W. AINSLIE HOLLIS, M.D. Cantab. This paper was read by Professor Humphry in the absence of Dr Hollis.

The antiquity and universality of the preferential use of the right hand was shewn by reference to the Biblical and other

records, and to Egyptian, Assyrian and other monuments, as well as to various members of the Semitic and Aryan groups of languages. All modern nations, with one or two questionable exceptions, are right-handed, and have words to signify " left-handed " corresponding with the French " gauche " and the Italian "mancino." It appears to be a peculiarity of the human race, even the Apes using the right and left limbs indiscriminately, and is associated with the higher and more elaborate muscular actions of the limbs in Man ; and there being no other structural difference between Man and the lower animals to account for it, the cause of the peculiarity must be sought in that part of the system, viz. the brain, in which he excels other animals. The left side of the brain was stated to be the larger in Man ; and it, through decussation of the nerve-fibres, presides over the right side of the body, and seems from recent observations also to preside over the complex and delicate muscular actions upon which articulation depends. The preponderance of the left side of the brain—the lopsidedness of the organ—thus engendered by the preferential use of the right hand, by the movements in speech and by much of subsidiary brain-work directly associated with speech, is not without its evil ; and instances were adduced, including those of Johnson and Swift, in which the left side of the brain had suffered and paralysis of the right side of the body had been induced, apparently, as a consequence of this overwork. The inference was drawn that such result might have been avoided had a more equal duty been required of the two sides of the brain by a more equal use of the two limbs ; and in these days of high pressure it is of especial importance to attend to such points, and by more equal education of the two sides of the body, to lead to a fairer distribution of work between the cerebral hemispheres.

Professor PAGET thought that more evidence should be adduced respecting the greater size of the left hemisphere of

the brain, and as to which side of the brain is likely to be affected when Aphasia occurs in left-handed persons.

Mr ANNINGSON questioned whether lopsidedness was really a part of right-handedness, forasmuch as the left hand is employed not only as a helpmate to the right, but for many purposes in which the right hand is less efficacious.

Mr CARVER thought the observations in the nursery shewed that right-handedness was acquired rather than innate; children having commonly a propensity to use the left hand, which it required some difficulty to counteract.

Professor HUMPHRY stated that the paper, which was one of much learning and interest, as well as suggestive, had been consigned to him for publication in the next number of the *Journal of Anatomy and Physiology*. In reply to various questions which had been asked, he said he believed an advantage gained by preferential use of the right hand, was a greater aptness and precision of movement requisite for delicate manipulations than could have been attained had both limbs been equally employed. Left-handed persons, being prevented by social custom from concentrating their attention on the left hand and being compelled to give a frequent preference to the right, are at some disadvantage in this respect. He could see no anatomical reason for the preference of the right limb, the slight advantage in circulation to the right arm through the innominate artery and vein applying, in nearly equal degree, to the right side of the brain. He agreed with Mr Carver that right-handedness was much a matter of education, and followed from the multifarious single-handed offices which are associated with the higher mental endowments.

(2) *On the Peritoneum in Man and other Vertebrates.*

Dr WILSON made a communication on the disposition of the peritoneum in Man and other vertebrata. He gave a brief account from his own dissections, of the anatomy of the peritoneum, and more particularly of its omental sac in Man and many Mammals, Reptiles, Amphibians and Fishes. He shewed that in many of these the omental sac is divided into two parts—a gastro-hepatic and a gastro-colic part—by a constriction corresponding with the upper border of the stomach. This he first observed in the dissection of a Narwhal, and had found it marked to a variable extent in Man, most evident in a young Hippopotamus, distinct in the Rat and in the human fœtus about the 3rd month. In Reptiles and Amphibians the omentum does not extend below the level of the stomach. There is therefore only a more or less complete representative of the gastro-hepatic part of the omental pouch of Man. One or more of the hepatic lobes usually project into the gastro-hepatic part of the sac. In Man it is the lobulus Spigelei. He described the relation of the spleen to the omental pouch, and stated that his observations were, on the whole, in accordance with the old and commonly received view regarding the mode in which the colon is embraced by the two recurrent layers of the omentum which pass on to form the transverse meso-colon.

Professor HUMPHRY remarked on the thorough manner in which Dr Wilson had investigated the anatomy of the omentum, which was of much interest with reference to the development of parts. The increasing size of the omental pouch in the higher animals and in Man must also be taken in connection with the recent investigations of Dr Klein respecting the relations of the peritoneal cavity to the lymphatic system.

The President (Professor Babington) in the Chair.

On the Centre of Motion of the Eye. By Prof. Clerk Maxwell.

The series of positions which the eye assumes as it is rolled horizontally have been investigated by Donders (Donders and Doijer, *Derde Jaarlijksch Verslag betr. het Nederlandsch Gasthuis voor Ooglijders.* Utrecht, 1862), and recently by Mr J. L. Tupper (*Proc. R. S.*, June 18, 1874). The chief difficulty in the investigation consists in fixing the head while the eyeball moves. The only satisfactory method of obtaining a system of co-ordinates fixed with reference to the skull is that adopted by Helmholtz (*Handbuch der Physiologischen Optik*, p. 517), and described in his Croonian Lecture.

A piece of wood, part of the upper surface of which is covered with warm sealingwax, is placed between the teeth and bitten hard till the sealingwax sets and forms a cast of the upper teeth. By inserting the teeth into their proper holes in the sealingwax the piece of wood may at any time be placed in a determinate position relatively to the skull.

By this device of Helmholtz the patient is relieved from the pressure of screws and clamps applied to the skin of his head, and he becomes free to move his head as he likes, provided he keeps the piece of wood between his teeth.

If we can now adjust another piece of wood so that it shall always have a determinate position with respect to the eyeball, we may study the motion of the one piece of wood with respect to the other as the eye moves about.

For this purpose a small mirror is fixed to a board, and a dot is marked on the mirror. If the eye, looking straight at the image of its own pupil in the mirror, sees the dot in the

centre of the pupil, the normal to the mirror through the dot
is the visual axis of the eye—a determinate line.

A right-angled prism is fixed to the board near the eye in
such a position that the eye sees the image of its own cornea
in profile by reflexion, first at the prism, and then at the mirror.
A vertical line is drawn with black sealingwax on the surface
of the prism next the eye, and the board is moved towards or
from the eye till this line appears as a tangent to the front
of the cornea, while the dot still is seen to cover the centre
of the image of the pupil. The only way in which the position
of the board can now vary with respect to the eye is by turning
round the line of vision as an axis, and this is prevented by the
board being laid on a horizontal platform carried by the teeth.

If now the eye is brought into two different positions and
the board moved on the platform, so as to be always in the
same position relative to the eye, we have to find the centre
about which the board might have turned so as to get from one
position to the other.

For this purpose two holes are made in the platform, and
a needle thrust through the holes is made to prick a card
fastened to the upper board. We thus obtain two pairs of
points, AB for the first position, and ab for the second.

The ordinary rule for determining the centre of motion
is to draw lines bisecting Aa and Bb at right angles. The
intersection of these is the centre of motion. This construction
fails when the centre of motion is in or near the line AB, for
then the two lines coincide. In this case we may produce
AB and ab till they meet, and draw a line bisecting the angle
externally. This line will pass through the centre of motion
as well as the other two, and when they coincide it intersects
them at right angles.

On the Formation of Mountains on the hypothesis of a liquid substratum. By Rev. O. FISHER, F.G.S.

This paper was a sequel to one read in December, 1873, in which it had been shewn that, upon the supposition that the inequalities of the earth's surface have been formed by contraction of its volume through cooling, they are too great to be so accounted for if the earth has cooled as a *solid* body. In the present communication it was therefore assumed that there is a *liquid* layer beneath the cooled crust. After discussing several of the hypotheses of geologists regarding the formation of continental areas and ocean-basins, and of mountain-ranges, an enquiry was made regarding the form which a flexible crust would take, if it rested in corrugations on a liquid substratum. The answer arrived at was, that a section carried across the corrugations would approximately present at the lower surface a series of equal portions of circular arcs, concave upwards, and arranged end to end in a festoon-like manner.

Their common radius would be $2\frac{\rho}{\sigma}c$, where ρ, σ are the densities of the crust and liquid respectively, and c the thickness of the crust. The horizontal pressure would be zero at the highest points.

In applying this result to the crust of the earth, it was admitted that it can give only a very approximate solution of the problem. Owing to the defect from flexibility, and from absolute fluidity in the substratum on which it rests, the conditions assumed would not be strictly fulfilled, and they would also vary from place to place, so that no uniform result could be expected. Nevertheless it seems tolerably certain that

the forms of the corrugations would be of the above general character, and that they would not consist of long-phased rolling undulations with flattened anticlinals, but rather of cusp-like elevated ridges, with lateral profiles somewhat circular. The horizontal pressure, though not actually evanescent, would be small at the highest, and greatest at the lowest points of the corrugations.

The radius of the curves which define the sides of the elevations, as given by the above expression, being constant, any additional lateral compression could not be met by an increase in the curvature of the corrugations; so that there would be a tendency to accumulate material about the anticlinals. This circumstance would account for the plications observed on the flanks of mountain-ranges. For the tendency to heap the material together in such situations would be met by its descending superficially by its own weight, until it attained the angle of repose, and in so doing the strata would become plicated. Such seems a more probable account of the plications, which are often on quite a small scale, than that they have been formed directly by the general compression of the crust.

Causes were suggested which might tend to lessen the compression in the neighbourhood of anticlinals, and admit of the extrusion of steam and lava from below. In connection with this point it was argued that the permanent state of fusion of the lava in certain volcanic vents, such as Stromboli and Kilauea, can be due to nothing else than the passage of intensely heated vapours through them; whence it would follow that any place in the earth's crust, which is not sufficiently firmly constituted to prevent the passage of steam at a high tension, might be sufficient to originate a volcano.

It was admitted that the larger features of continental plateaux and oceanic depressions had been so far left unexplained.

March 8, 1875.

Professor STOKES in the Chair.

The following communications were made by Mr W. T. KINGSLEY,

(I)   *On the cause of the " wolf" in the Violoncello.*

Mr KINGSLEY said that the " wolf" occurs somewhere about the low E or E flat, and was attributed to the finger-board having the same pitch, so that the finger-board becomes as it were a portion of the string stopped down on it and vibrates with it: if this is the true cause, the " wolf" cannot be got rid of, but may be placed at such a pitch between E and E flat as to occur on a note rarely used: also by thickening the neck of the finger-board, the extent of discursion in the vibration may be made less.

The MASTER OF ST CATHARINE'S COLLEGE remarked that a different explanation of the phenomenon was given by M. Savart, which was to this effect. The old Italian makers constructed the violoncello of such dimensions that the mass of air included within the instrument resonates to a note making 85·33 vibrations in a second, a number which then represented the lowest F on the C string, but which now, owing to the rise of pitch since the beginning of the 18th century, nearly represents the note E immediately below it. Savart's theory was that notes half a tone above or below this E will cause beats between the vibrations of the string and those of the mass of included air. It seemed quite possible that the mass of air contained in the instrument should be capable of controlling the vibrations of the whole instrument, but not that the vibrations of the finger-board alone (as Mr Kingsley suggested) could do this. For the sound, technically called the " wolf," is an actual

check to the whole vibration of the violoncello, producing not merely beats, but a baying sound, destitute of the freedom of vibration which characterizes other notes.

But a great objection to the above explanation is this experiment. On an Italian instrument, the upper D on the 4th or lowest string is the imperfect note. But when the same note is elicited from the 3rd string, the note is perfectly resonant. This peculiar effect seems then to depend upon the point of the finger-board which is pressed. It is also well known that the "wolf" can be modified by an alteration of the position of the sound-post. As an explanation, we may conceive that the whole framework of the violoncello vibrates like a stretched string, producing its fundamental, with a series of overtones, and that a nodal line passes through the point of the finger-board, pressure upon which produces the "wolf," and that thus all vibrations being destroyed except those which have a node at the point of pressure, this peculiar tone is elicited.

Mr TROTTER said that if Mr Kingsley's explanation of the cause of the "wolf" was the true one, it was to be expected that it should be produced when a certain note was sounded upon one string and not upon another. The fingering would be different in the two cases, and the note to which the finger-board responded would vary with the point touched by the finger.

(2) *A description of the Instruments used in sounding some of the Lakes in the Snowdon District, and an account of the results obtained.*

Mr KINGSLEY gave a description of the Plummet, Registering Apparatus and Protractors used by him in sounding several of the deep lakes in the Snowdon district last June.

The plummet is a modification of the deep-sea plummet now generally used, the principal alteration being in the appli-

cation of a heavy gouge to aid in bringing up specimens of the bottom.

The recording apparatus is a modification of the paying-out apparatus used for laying deep-sea telegraph cables.

The protractors are diagonal telescopes mounted on bars revolving on vertical axes, and having fiducial edges radiating from the centres of the axes.

One protractor is placed at each extremity of the base on a horizontal table, on which is strained a sheet of drawing paper; the telescopes are first collimated with each other, and then a line is drawn by the fiducial edges on each sheet of paper; the boat with the surrounding apparatus is followed by the two observers at the protractors, and when a signal is given, a line is ruled and numbered by each observer; finally the two papers are placed so as to have the lines of collimation in coincidence and the centres at the scale distances apart; then by looking through the papers and pricking the intersections of the corresponding lines, the positions of the boat are laid down on two maps.

In practice this is all done easily, and no particular skill is needed in the observers with the protractors.

The results obtained shewed that the bottoms of these lakes are comparatively flat, the greatest depths being reached at a short distance from the shore on the cross section, and occurring also nearer to the upper end of the lake than to the lower: the forms of the bottoms correspond in a remarkable manner with the set that would be given to glaciers descending into the hollows in which the lakes lie; and Mr Kingsley believed them to have been formed by the action of glaciers during the extreme cold or penultimate glacier epoch; because in one case, that of Llyn Cawlyd, the lake lies almost on a watershed, where no glacier could now form, but which was a depression forming a lateral outflow from the great glacier that at one time filled the whole hollow between the Glydyrs and Carnedds;

during the last glacier epoch most of these hollows were again filled with ice to a great height, but these last glaciers were comparatively small.

Mr Kingsley especially dwelt upon the difficulty of disentangling the scattered moraine from the drift, and also of distinguishing between the striations belonging to the two cold epochs.

Professor HUGHES, in a discussion with Mr Kingsley, explained and defended Professor Ramsay's theory of the glacial erosion of lake basins, but was not prepared to go so far as that author in his application of it. He said we wanted some more definite information as to the shearing force of ice : there was a limit to the possible depth to which ice could descend and pass out without being embayed. The observations in "roche moutonnée," where the surrounding ice corresponded to the sides of the rock-basin, shewed that ice could descend to a considerable depth without being embayed ; and it had to be shewn in each separate case whether it was possible or probable that ice had scooped out, or been embayed in, any particular depression. He did not think there was evidence to shew that glacial conditions were synchronous in the Northern and Southern hemispheres, or even in Europe and America.

Mr HILL described a method by which a "camera obscura" may be employed to fix the position of the boat taking soundings.

Professor MAXWELL stated that piano-wire furnished the best means of suspending the sounding-plummet. He mentioned some other improvements in the methods used for taking soundings.

Mr BONNEY said that the facts collected by Mr Kingsley were very valuable, because the need of careful and accurate soundings was so much felt in discussions as to the origin of lake-basins. He was himself of opinion that the erosive force of glaciers was but small, and that they had not excavated

such lakes as those of Switzerland and Italy. From his own investigations in the Alps and Britain he was of opinion that a glacier could only erode a rock-basin under certain exceptional circumstances. After describing these, he stated that he was inclined to refer the rock-basins about Snowdon to the later period of glaciation. He also thought that such causes as change in the eccentricity of the earth's orbit, the effects of precession, and the alteration of sea and land, were more likely to have caused the glacial period than any variation in the sun's heat.

Mr ELLIS enquired how Mr Kingsley obtained the true depth when wind was blowing, and therefore the line not vertical, owing to the drifting of the boat.

April 19, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

(1) On the Mode of Formation of the Alimentary Canal in Vertebrata. By F. M. BALFOUR.

The author stated that the simplest type of vertebrate development was that exhibited by Amphioxus, and that all the complicated types found amongst other vertebrates were to be looked on as derivatives from this single type.

He shewed that in Amphioxus the alimentary canal was formed by a simple invagination; that in the Frog the invagination had ceased to be symmetrical and single as in Amphioxus, but had become unsymmetrical and had acquired other peculiar characters.

In the Selachian's development, which was to be looked upon as the type most allied to that of the Frog, the invagination was no longer present, but traces of it still remained. The

other features of development were exaggerations of what oc-
curred in the Frog.

From the Selachian to the Bird the author pointed out that
there was a wide gap which he could not satisfactorily fill up.

The author, in conclusion, drew attention to the food-yolk
in the eggs of most vertebrates, which he said was to be looked
upon as the most important agent in producing the modifica-
tions which he had described.

(2) *On the Physiological Action of Jaborandi.* *By*
J. N. LANGLEY, St John's College.

May 3, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

*On a method of introducing a Current into a Galvano-
meter Circuit.* *By* Mr PIRIE, Queens' College.

Mr PIRIE said that electricians had often to work with
currents far too strong for their galvanometer. He mentioned
various methods in use for checking the swing of the needle;
but contended that an easily made and easily used controller
for rough work was a desideratum. He described an instru-
ment in the form of a continuously varying shunt, in which a
moving connection was obtained by a tube filled with mercury
sliding on a wire of suitable resistance. This form of connection
was first used by Mr Barrett of Dublin.

With the aid of Mr GARNETT, the Demonstrator of Physics,
Mr Pirie shewed that a very good connection was obtained by
this means; and subsequently, that the instrument described
gave a control over the movements of the needle in a galvano-
meter whose resistance was not too different from its own.

May 17, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The PUBLIC ORATOR made a communication

On the place of Music in Education as conceived by Aristotle (Politics v. [VIII.] cc. 3—7),

of which the following is an abstract :—

Gymnastic, Grammar and Drawing, considered as branches of education, have direct practical utilities ; but it might be doubted, Aristotle says, what is the use of Music. Three objects might be assigned to the study of music : παιδεία, discipline ; παιδιά, pastime : διαγωγή, the rational employment of leisure. On further inquiry we see that (leaving παιδιά out of account) the serious uses of music are these three—παιδεία, διαγωγή, and κάθαρσις, the purification of the emotions. I. παιδεία. The disciplinary value of music is (*a*) artistic, as training the perceptions, and so preparing διαγωγή: (*b*) moral, as establishing the faculty of rejoicing aright—ἐθίζουσα δύνασθαι χαίρειν ὀρθῶς. For, while forms or colours are merely symbols (σημεῖα) of character or feeling, musical sounds may be images (ὁμοιώματα); and pleasure in the imitative expression will create sympathy with the feelings imitated. But does music, as a part of early training, imply the power of performing upon any instrument ? Aristotle holds that it does, since a certain measure of practical knowledge is necessary to make a competent critic : only, in order to guard against τὸ βάναυσον, the pursuit of executive skill must be limited by two things ; first, the study of music must not interfere with other studies ; secondly, the body of the citizen must not be unfitted for war or for those exercises which befit free men : the study of music

must stop short of what is τεχνική. II. διαγωγή. As the practical is subordinate to the speculative reason, so work is subordinate to rest; and the aim of education is to teach men, first, how they shall procure, next, how they shall use, leisure. Here, then, is the reason of the place held by music in the mature life of the normal citizen—it is one of the noblest and most elevating employments for leisure—ministering, in that quality, to two special purposes,—the culture of the intelligence (φρόνησις), first, by relaxation, then by a gentle exercise of the critical faculty in alliance with the imagination;—and the purification of the emotions. III. κάθαρσις. In the Poetics Tragedy is described as δι᾽ ἐλέου καὶ φόβου περαίνουσα τὴν τῶν τοιούτων παθημάτων κάθαρσιν. Four principal explanations of 'katharsis' have been suggested :—(1) that 'moderating' of the emotions which might arise from familiarity with such objects as excite them: (2) 'chastisement of the *bad* passions'—an explanation which is at variance with Aristotle's language, since it excludes pity and terror themselves from the number τῶν τοιούτων παθημάτων : (3) 'the separation from pity and terror of what is disagreeable in such emotions when excited by *real* objects, and not, as in Tragedy, by fiction';—a view to which it may be objected that the work of κάθαρσις is manifestly something gradual, and, in its effect, lasting, not something confined to a momentary impression ; it is a 'healing' of the soul : (4) 'the correction and refinement of the passions.' Twining says : 'the doctrine, therefore, of Aristotle...would perhaps only amount to this—that the habitual exercise of the passions by works of imagination in general of the serious and pathetic kind (such as Tragedies, Novels, &c.) has a tendency to soften and refine those passions when excited by real objects in common life.'

This view seems essentially modern. It may be doubted whether the idea of '*softening, refining*,' &c. had anything to do with the notion of κάθαρσις. Rather, probably, it means

'clarification'—*i.e.* presentment in typical clearness, with everything accidental or confusing withdrawn—in that *Heiterkeit* and *Allgemeinheit* which, as Winckelmann says, are the two characteristics of the Greek ideal. As in Sculpture, as in Tragedy, so in Music, this process would be a means, not necessarily of softening, but often of intensifying, by the simple and concentrated expression of feelings or issues from which the vulgar, the spurious, the petty, the falsely sentimental have been detached. What Tragedy does in the sphere of action, πρᾶξις, this Music does in the sphere of that normal imitation, ἠθικὴ ὁμοίωσις, which is its own. Aristotle's view as to the universal moral importance of music as an element in education seems to be enforced by some of those phenomena of the present day which shew how a repressed and uneducated sensibility may become ungovernable: though, since music has been set on a really scientific basis, his plea that it is necessary to be a performer in order to be an intelligent listener has no longer its Greek validity.

May 31, 1875.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following paper was read by Mr F. M. BALFOUR, of Trinity College.

On the Segmental Organs of Vertebrates.

The author stated that the recent investigations of Professor Semper and himself had led to the discovery that in the selachians the kidneys were developed from a series of primitively independent structures. Each of these was a tube opening at one end into the body-cavity and ending blindly at the other. These tubes corresponded in number with the proto-

vertebral segments. As development proceeded, the blind end of the most anterior of these became elongated, and gradually acquired a connection with each of the posterior tubes in succession, and finally at its posterior extremity opened to the exterior. The author then entered into further details as to the changes which these parts subsequently underwent, and attempted to demonstrate the homologies between the kidneys of selachians and those of the higher vertebrates.

He concluded by pointing out that the tubes he had described bore such a striking resemblance to the segmental organs of annelids that in his opinion the identity of the two structures was certain. It followed from this (1) that the ancestry of vertebrates was to be looked for in the annelids; (2) that the vertebral segments of the vertebrates were to be looked upon as similar to those of annelids, and not, as had sometimes been said, as due to a secondary segmentation.

NEW FELLOWS, &c.

During the Academical years 1873—1875. the following New Members of the Society were elected.

Honorary Members.

Mar. 16, 1874.	Col. J. T. WALKER, R.E., F.R.S.
April 27. ..	Prof. A. T. ANGSTROM. Upsala, (since deceased).
,, ,,	M. CHEVREUIL. Paris.
,, ,,	M. OTTO VON STRUVE. Pulkova.
,, ,,	Prof. W. E. WEBER. Göttingen.
Feb. 22, 1875.	Dr T. ANDREW. Belfast.
,. ..	Dr F. C. DONDERS. Utrecht.
.. ..	W. K. PARKER, F.R.S. Hunterian Professor.
.. ..	Sir W. R. GROVE, M.A., F.R.S.
.. ..	Prof. J. C. POGGENDORF. Berlin.

Fellows.

Oct. 26, 1874.	Prof. COWELL, M.A., Corpus Christi College.
Nov. 16, ,,	OSBERT SALVIN, M.A., Trinity Hall.
.. ,,	G. J. ROMANES, M.A., Caius College.
Feb. 8, 1875.	Rev. A. ROSE, M.A., Emmanuel College.
,. ,,	F. H. NEVILLE, M.A., Sidney Sussex College.
Feb. 22, ,,	Rev. D. B. BANHAM, M.A., Caius College.
,, ..	J. G. RICHARDSON, B.A., Trinity College.
May 17, ,.	H. F. BANHAM, M.A., St John's College.
,, ,,	D. BURGESS, B.A., Corpus Christi College.
May 31, ,,	G. CHRYSTAL, B.A., Corpus Christi College.

Associates.

Nov. 3, 1873.	Mr A. DECK.
Mar. 16, 1874.	Mr H. BAXTER.
Feb. 22, 1875.	Dr ARMITSTEAD.
,, ,,	Mr BOWES.

(PART XVI.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

ANNUAL GENERAL MEETING,

OCTOBER 25, 1875.

ERRATA (No. XV.)

P. 355, l. 8, *for* "ones" *read* "axes."
P. 376, last line, *for* "classification" *read* "clarification."

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On some Fresh Observations of the Water-holes on the Gorner Glacier. By Mr TROTTER.

The attention of the Society was called last year to certain water-holes on the Gorner Glacier associated with hummocks of ice on their southern edges (first observed by the speaker in 1863). (See Proceedings, Oct. 19, 1874.)

The holes as they appeared at the latter part of the season were oval, with their longer axes pretty exactly east and west, the larger axis about double of the smaller, the depth on an average nearly double the longer axis, the longer axis varying from about 1' to 6' or 8' long. The holes as usual had gravel at the bottom, and had usually a hummock of ice at the southern side, the height of which was often nearly equal to the longer axis in the smaller holes, somewhat less in proportion in the larger ones.

The larger axis was sometimes parallel, or nearly so, to the veined structure, sometimes cut it at a greater or less angle, so that the holes were clearly independent of the veined structure, and seemed to be clearly a meridian phenomenon.

The speaker had the opportunity of observing these holes last summer, about the end of June, when they were much less perfectly formed. The surface of the glacier was covered in places with new snow, and in others the winter's snow was imperfectly melted. Some of the holes, however, were fairly

30—2

developed, though of course not so deep as they were later on in the year. Others were of much less regular form: some had the hummock on the south side well developed; others had no perceptible hummock, but seemed to have more or less of a raised margin all round; others seemed to show a slight hummock on the north as well as the south side.

The explanation of the phenomenon which was suggested was as follows :—A collection of gravel gives rise to an irregular or roughly circular shallow water-hole, the water being at first at a considerably lower level than the edge of the hole, formed in part of snow and soft ice.

(1) Towards noon the sun's rays are incident upon the surface at a small angle, a comparatively small portion are reflected, and the radiations which enter the water are for the most part absorbed before reaching the wall of the hole, and the resulting heat is carried to the bottom by the descending current of dense water, whose temperature has been raised above the freezing point. This melts ice at the bottom and deepens the hole. On the other hand, towards morning and evening the angle of incidence is larger, a larger proportion of the rays are reflected and strike upon any portions of the east and west boundaries of the hole which are above water. The rays which enter the water have a shorter path to traverse in water before reaching the wall, and therefore will reach and melt it in a greater proportion. The east and west walls will thus be more melted than the south wall, and the hole gradually assumes its oval shape with its longer axis east and west.

(2) The ice in the neighbourhood of the holes is for the most part of very rough and irregular surface, very pure, and with small bright crystalline faces inclined in all directions. Consequently a considerable portion of the rays incident on any portion of it, and there reflected, will strike another portion of the surface, so that the wasting of any portion of the surface is due not only to the rays primarily incident on that portion,

but also to those incident upon it after reflection at another place. On the other hand, rays falling upon the water will be either absorbed or reflected regularly, so as to pass clear of the ice, except possibly the actual wall of the hole. Hence the ice in the immediate neighbourhood of the hole will receive less radiation, and therefore be less melted than the rest of the surface, and there will be a tendency to the formation of a raised rim surrounding the water-hole.

(3) As fast however as this rises, those portions of it which are on the north, east, and west sides of the hole will be melted, as they will at some part of the day receive the sun's rays not only on their upper surface, but on the vertical face towards the hole. The east and west sides however will be most attacked, for reasons given above in (1).

The final result will therefore be an oval hole with its major axis east and west, with a marked hummock of ice on its south side, and sometimes traces of one to the north.

(4) The most serious difficulty in the way of this explanation seems to be in the local nature of the phenomenon. Why is it not produced wherever there is a level surface of glacier? The ice where the phenomenon is conspicuous is of a peculiar soft nature, full of minute air-bubbles, which give it an unusually white appearance. Ablation probably takes place rapidly over the surface, so that phenomena depending upon differential ablation are conspicuous. The peculiar kind of irregularity of the surface would favour the action described in (2).

[*Communicated Nov.* 15, 1875.]

Since the notice of my paper read on Oct. 18th, 'Further remarks on the water-holes of the Gorner Glacier,' was published, my attention has been called to a passage in Agassiz' 'Nouvelles études sur les Glaciers,' &c., Paris, 1847, p. 101—2, in which a similar phenomenon is described as having been observed by Dr Ferdinand Keller. The account of the phe-

nomenon observed by Dr Keller on the Aar Glacier so closely resembles that given by me of the water-holes on the Gorner Glacier, that there can be no doubt that they refer to the same phenomenon, and that therefore it was first noticed by Dr Keller, and described by him in 1847.

I cannot however think that Dr Keller's explanation of the phenomenon is satisfactory. He speaks of the holes as semicircular with the arc towards the north, and attributes the greater depth of the northern portion of the hole to the longer time for which the sun will fall upon the gravel on the north portion of the bottom.

This does not explain the much more striking phenomenon, the east and west elongation of the hole. Moreover, the holes are so deep in the latter part of the summer that the gravel at the bottom must be in shade all day. No explanation is given of the accompanying hummocks of ice, which are spoken of as if they existed before the water-hole, whereas my observations of last summer make it clear that they are formed subsequently. I therefore still adhere to the explanation given in my paper, which seems to me to explain all the phenomena.

October 25, 1875.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On Herwart ab Hohenburg's Tabulæ Arithmeticæ προσθαφαιρεσέως universales, Munich, 1610. By J. W. L. GLAISHER, F. R. S.

The title more at length is "Tabulæ arithmeticæ προσθαφαιρεσέως universales, quarum subsidio numerus quilibet, ex multiplicatione producendus, per solam additionem : et quotiens

quilibet, e divisione eliciendus, per solam subtractionem, sine tædiosâ & lubricâ Multiplicationis, atque Divisionis operatione, etiam ab eo, qui Arithmetices non admodum sit gnarus, exactè, celeriter & nullo negotio invenitur.   E museo Ioannis Georgii Herwart ab Hohenburg…Monachii Bavariarum…Anno Christi, M.DC.X.", and the book is a very large and thick folio.   It contains a multiplication table up to 1000×1000, the thousand multiples of any one number being given on the same page ; and there is an introduction of seven pages, in which the use of the table in multiplying numbers containing more than three figures, and in the solution of spherical triangles, is explained.

Very little information about the work is to be obtained from the mathematical bibliographers and historians.   Heilbronner (*Hist. Math.* 1742, p. 801) gives the title not quite correctly, and adds "Docet in his tabulis sine abaco multiplicationem atque divisionem perficere."   Kästner (*Gesch. der Math.* 1796—1800, t. iii. p. 8) quotes the title from Heilbronner and his remark, and adds that the latter could not have known Herwart's method, or he would have described it.   He remembers to have read somewhere that the book contained a number of tables of products, arranged by factors, like a great multiplication table.   Scheibel (*Einl. zur math. Buch.* 1775, t. ii. p. 417) gives the title-page correctly, and explains the method of using the table when the number of figures in the multiplier or multiplicand exceeds three, and concludes with the remark, "So viel von diesem ungeheuren Folianten, den man bloss zur Curiosität und seiner Seltenheit wegen, in einer mathematischen Büchersammlung aufbewahret."   Montucla (*Hist. des Math.* t. ii. p. 13) gives a description of the mode of using the table, remarking that but for the invention of logarithms it might have been of use to calculators, supposing the labour of searching for the products in so large a folio not to be more fatiguing than the direct performance of the work.   Murhard

(*Bibl. Math.* 1797—1804, t. ii. p. 199) gives the title correctly, and marks it with an asterisk to show that he has seen the work himself. Rogg (*Bibl. Math.* 1830, p. 142) merely has, "Hohenburg, Gregor (sic) Herwardt ab, tabulæ arithmeticæ προσθαφαιρεσεως universales, 1610." Neither Weidler (*Bibl. Ast.* 1755), Deschales (*Cur. seu Mund. Math.* 1690), Lalande (*Bibl. Ast.* 1803), nor Delambre (*Hist. de l'Ast. mod.* 1821), mention the work; but there is a reference to it in Leslie's Philosophy of Arithmetic (2nd edit. 1820, p. 246). In his article on tables in the English Cyclopædia (1863) De Morgan wrote, "The table goes up to 1000×1000, each page taking one multiplier complete. There are then a thousand odd pages, and as the paper is thick, the folio is almost unique in thickness. There is a short preface of seven pages, containing examples of application to spherical triangles. It is truly remarkable that while the difficulties of trigonometrical calculation were stimulating the invention of logarithms, they were also giving rise to this the earliest work of extensively tabulated multiplication. Herwart passes for the author, but nothing indicates more than that the manuscript was found in his collection. The book is excessively rare, a copy sold by auction a few years ago was the only one we ever saw." Graesse (Trésor de livres rares, 1859—1867) says that by the book the use of logarithms was first spread in Germany, which is of course erroneous.

Herwart was Chancellor of the Palatinate of Bavaria, and published several other works (the most complete list is in the Bodleian Catalogue), among which his "Ludovicus IV. imperator defensus (Munich, 1618—19)" is the best known. In the *Biographie Universelle* it is described as still useful for the history of Germany; and Scheibel speaks of Herwart as " der berühmte Staatsmann und Geschichtschreiber."

While I was engaged in preparing the report of the British Association Committee on mathematical tables, I endeavoured

without success to find something beyond what is quoted above about the table; but the hope is there expressed that, considering the attention so large a work must have received from contemporary mathematicians, some information might still be gained with regard to the calculator of the table, his objects, &c.

I recently found a correspondence of six letters between Herwart and Kepler, which took place at the end of 1608, with regard to the table, and which throws light upon these points. The letters are printed in Dr Frisch's 'Joannis Kepleri Astronomi opera omnia' (t. iv. pt. II. pp. 527—530, 1863).

In the first letter, dated September 13, 1608, Herwart writes, "Ich hab bisher in Multiplicatione et Divisione sonderbare geschriebene praxin gebraucht, dadurch ich den numerum ex quavis multiplicatione productum, per solam additionem, und den Quotienten ex divisione resultantem per solam subtractionem (absque tediosa multiplicationum et divisionum operatione) gefunden." He states that J. Prætorius and others who have seen it recommend him to have it printed, and he adds that if he had not had this method, on account of his continual occupations and because he is not a good calculator, he should long ago have had to give up all mathematics that required calculation. He sends a specimen page of the table, the use of which he explains, and he prays Kepler to give him his opinion on the matter without delay.

Kepler replies on October 18, 1608, and remarks that 1000 pages will make a large volume, which the computer will often not have at hand. He suggests that short precepts on the solution of triangles should be added, as Herwart's table would often be preferable to the '$\pi\rho o\sigma\theta\alpha\phi\alpha\iota\rho\epsilon\sigma\iota\varsigma$ Vitichiana,' which is too elaborate to be retained in the memory, confuses sines and their complements, &c. Besides, the reasons for the operations are hidden in work, "At si multiplicemus et dividamus simpliciter, tunc videmus quid agamus; et possunt varietates trian-

gulorum talibus præceptis comprehendi, quæ memoria retineri facile possunt." Kepler then gives a synopsis of the sixteen cases of the solution of spherical right-angled triangles.

Herwart writes on November 5, and says he had found that the triangles could be solved better by help of his table than by prosthaphæresis, so that Kepler was quite right. He mentions that he has not seen Vitichius. As it is usual to prefix a 'splendid title' to books, that they may sell, he suggests the following, " Nova, exacta, certa et omnium facillima ratio Arithmetices, per quam numerus ex multiplicatione productus sine operatione multiplicationis per solam additionem, et quotiens ex divisione resultans absque operosis ambagibus divisionis per solam subtractionem cujuscunque, etiam maximæ summæ, etiam ab eo qui arithmetices non admodum sit gnarus, citius quam ulla alia ratione invenitur." He then prays Kepler to send soon his advice as to how the table should be entitled.

Not receiving an answer on December 2, he writes a short letter to Kepler again asking for a reply, and suggesting that perhaps it was not prudent ' so speciosum titulum tantillæ rei zu præ figiren.' But in the mean time a letter from Kepler had been received, and, writing on December 5, Herwart explains that he does not expect an answer to his last letter, and that he understands that Kepler has no objection to the title, but thinks it ought to be shortened. He cannot understand the meaning of Kepler's advice, 'Græca compositio imploranda, sed exercito (sic),' and asks for an explanation.

In the last letter of the correspondence, dated December 12, 1608, Kepler explains that as the title seemed long, he had advised that it should be shortened by the composition of two Greek words as ἐρυσιπελας, ριψοκινδυνος, and as a suitable word did not occur to him he had suggested that some one practised in Greek should be consulted. But perhaps a good idea has occurred to himself. Σεισαχθεια ἀριθμητικη. " Nosti enim, χρεων αποκοπας sic dici. Inest vocabulo et emphasis et pro-

prietas et similitudinis gratia, quia me Hercule novas tabulas introducis, et uno ictu liberas computatores debitis multiplicandi, et dividendi inextricabilibus.' But he hopes that this title will not offend Maginus on account of his 'Tabula tetragonica' [Venice, 1592]. At the end he adds the postscript, "Titulus igitur talis: Σεισαχθεια, sive Novæ Tabulæ, quibus Arithmetici debitis inextricabilibus multiplicandi et dividendi liberantur, ingenis, tempori, viribusque ratiocinantis consulitur."

It is thus proved that the table was printed from a manu-script which Herwart used himself, and which very likely he had had made. The correspondence is of interest, as the table, regarded simply as a multiplication table, has never been surpassed in extent, and has only been equalled by Crelle's *Rechentafeln*, first published in 1820, in two volumes, and which, now sold in one volume folio, is one of the best known and most used tables. Scheibel and others who have ridiculed Herwart's project were only right in so far as the great size of the work renders it unmanageable, as the use of a multiplication table up to 1000×1000 has, in spite of loga-rithms, been found to be both practicable and convenient. Herwart's work is very rare, but there are copies in the British Museum, the Bodleian Library, and the Graves Library at University College, London. It was this last (not quite perfect) copy which, through the kindness of Professor Henrici, I was enabled to exhibit to the meeting.

With regard to the word prosthaphæresis, it is well known that the prosthaphæresis of the orbit was the angle subtended by the eccentricity at the planet, and De Morgan explained the use of the word on the title-page thus: "Prosthaphæresis is a word compounded of prosthesis and aphæresis, and means addition and subtraction. Astronomical corrections, sometimes additive and sometimes subtractive, were called prosthaphære-ses. The constant necessity for multiplication in forming pro-portional parts for the corrections, gave rise to this table, which

therefore had the name of its application on the title-page."
But the prosthaphæresis referred to seems most likely a method
of solving spherical triangles, in which the product of two sines,
or of a sine and cosine, &c., is avoided by the use of formulæ, such
as $\sin a \sin b = \frac{1}{2} \{\cos (a - b) - \cos (a + b)\}$. This explains all
Kepler's allusions to prosthaphæresis, and as Herwart proposed
as the chief use of his tables to solve spherical triangles by direct
multiplication without previous transformation, as set forth in
his introduction, it justifies completely the use of the word on
the title-page.

Wittich was for a short time an assistant of Tycho
Brahe, and his method of prosthaphæresis appears to have been
a method of solving triangles so as to avoid multiplications by
means of formulæ, such as that just written, but I hope to
examine the matter more fully. Laplace (*Jour. de l'école polyt.*
Cah. xv. t. viii. 1809), referring to the same formula,

$$\sin a \sin b = \frac{1}{2} \{\cos (a - b) - \cos (a + b)\},$$

remarks that "cette manière ingénieuse de faire servir des
tables des sinus à la multiplication des nombres, fut imaginée
et employée un siècle environ avant l'invention des logarithmes"
(see Brit. Ass. Tables Report, p. 23, 1873).

November 1, 1875.

Professor Babington, Vice-President, in the Chair.

The following communication was made to the Society:

On Aristotle's notion of 'Right-Handedness'. By Mr Pearson.

After referring to the paper by Dr Hollis on this subject,
communicated last year (Nov. 30, 1874), the speaker stated
that he had been led by Aristotle's great reputation to enquire

what his views on the subject might have been. Partly from a perusal of much that Aristotle has written on the subject, but mainly from the new Index by Prof. Bonitz, he gave a *résumé* of the passages bearing on the subject : these passages seemed to shew that Aristotle considered, (1) that the right hand or side was naturally the source or origin of motion, (2) that in nearly all living creatures capable of motion it is the better or stronger side, (3) but that while the heart is always the origin of vitality, it is in the human race only set towards the left side of the body ; in all other living creatures it is in the centre of the body or trunk (ἐν μεσῷ κεῖται τοῦ ἀναγκαίου σώματος). And though it may be a fair question how far Aristotle was misled by the preferential use of the right hand by the human race to attribute an excellence in the right side to the animal world, there can be no mistake about the distinct language in which he does so.

The passages referred to by Mr Pearson were as follows :

Περὶ Ζώων Μορίων, II. 2 ; III. 3, 4, 5 ; IV. the whole section, especially n. 8.

Περὶ Ζώων Ἱστορίας, I. 17; II. 1. 17.

Περὶ Ζώων Πορείας, XIX.

Nicom. Eth. V. 7 (10).

*Magna Moral. I. 34.

Politics II. (9) 12.

*Problems V. 37 ; VI. 5 ; XXXI. 12, 13.

De Respir. 16.

Plato, *Legg.* VII. 795.

Macrobius VII. 14.

Some observations were added about the terms in which Mr Lewes in his work on Aristotle (1864) criticizes some errors into which that writer has fallen in his works on Natural History, while it was admitted that he is probably right in considering that there is no reference in Aristotle's writings to the anatomical examination of any but animal subjects : though the

fact that Macrobius (VII. 14) ascribes to Erasistratus and Herophilus, two celebrated physicians of the succeeding generation, the practice not only of dissection but of vivisection of human bodies, shews, if the story is true, that public opinion could not have been quite unprepared for it.

Mr Pearson also referred to a passage in the *Encyc. Britan.*, art. *Comp. Anatomy* (§§ 202—205), ed. 1810 (but not occurring in later editions), in which the preferential use of the right hand is discussed, and ascribed to a natural peculiarity in the form of the sub-clavian and carotid arteries on that side : and in which it is stated that a similar preference for the right side may be traced in some dogs if not in horses. The speaker said, however, that he would not answer for the existence of such a preference himself in those animals, nor in the lion and camel, to which Aristotle (and Pliny after him) especially ascribe it. He concluded by exhibiting a lobster ; a kind of shell-fish of which the right claw is distinctly larger and stronger than the left, as is specially mentioned by Aristotle (οἱ καρκινοὶ...τὴν δεξίαν ἔχουσι χηλὴν μείζω καὶ ἰσχυροτέραν...).

In conclusion an opinion was expressed that the preferential use of the right arm might be due to a natural shrinking from the use of the side nearest the heart, or perhaps a natural wish to protect it as being, to our own sensations at least, the seat of vitality. At any rate such a use must be accepted as a fact, whether the more strictly anatomical reasons given in the former paper were correct or not, and the difficulty of coming to a conclusive view on the subject was suggested as a reason why the point is so little discussed, at any rate in the more popular and simpler treatises on anatomy : nor had the speaker been successful in finding much information on another question : viz., the true position of the heart in animals, Aristotle's view on this point being very decided, while the practice of dissecting them was evidently common in his time : but still he ventured to think that the situation of the centre of gravity of the human heart

to the left of the centre of the body had been a sufficient determining cause in favour of the preferential use of the right arm by our species.

Mr NEVILLE GOODMAN said that while he had listened with pleasure to the elucidation of Aristotle's ideas on the causes of right-handedness he thought these indicated that the inductive method was preferable to the speculative. These ideas shewed how possible it was for an ingenious man with the great repertory of nature before him to adduce many facts to support any theory once formed. No doubt there were many facts which would support the idea that the right side had a preferential motor function. In addition to those named, the whole order of Gasteropods might be quoted, in which in the vast majority of instances the opening of the generative organs, the vent and the respiratory chamber, were on the right side. In fact an ordinary snail or whelk exhibited the phenomenon of an excessive development of the right side, which excessive development had the effect of thrusting that side continuously over the other so as to result in the dextral helix. There were, however, many exceptions to this rule, as the *Funis Contrarius* of the Red Crag and numbers of existing shells, Clausiliæ, &c. Flatfish (Pleurovectidæ), the only animals of the vertebrate type markedly asymmetrical in their organs of relation, were by no means constant in having the right side of stronger motor function than the other; the upper, coloured, and more convex and muscular side being in many cases on the left. In the sole the developed side is usually the right, in the turbot usually the left.

With regard to the prior motion of the right side from a state of rest he had narrowly observed horses' paces and thought there was no ground for this supposition. The leading leg in the horse's canter became so purely from training. Both dogs and young horses constantly change the leading leg when running unrestrained. All the figures in Egyptian Art had

their left leg advanced in conformity with Aristotle's remarks, but he had regarded this attitude as purely conventional.

The heart of all mammals was of like asymmetrical position to that of man, and its position on the left side was more apparent than real; the butt end being directed to the right, and its apex or lower end to the left. The beating of the apex on the left side was caused by rhythmical distension of the aortic arch, which thus became periodically straightened, and in re-laxing the apex fell back on the left side of the thorax. It was quite possible and probable that, owing to the depression of the human thorax, (i.e.) its greater lateral than fore and aft diameter as compared with the compression of the same part in the lower animals, the apex of the heart might be thrust more to the left side, but he thought that the centre of gravity of the heart occupied a similar position in both cases.

With regard to the cause of right-handedness, as it could not be due to external conditions, it seemed reasonable to attri-bute it to the asymmetry of the internal organs, but he thought that the stomach, whose cardiac end was on the left side and which when distended with food bulged to that side, had a greater claim for consideration. The greater proximity of the right hand to the source of arterial blood through the innom-inate trunk, was also worthy of consideration. The greatest argument against accounting for right-handedness by any of these methods was that while cases of reversal of the whole viscera were known the individuals thus characterized were not left-handed. Right-handedness was so universal throughout the human race, that he thought it could not be accounted for by early education, the customs of the world in all conventional matters being so various. If due neither to asymmetry of the organs of nutrition nor to education, it must be an inherited instinct, accidental in the sense of being due to a cause which has now no bearing on the species. Such a meaningless and persistent habit would go far to prove the unity of the human

race. It would be very interesting to observe whether the *Quadrumana* shewed any preference in the use of the right hand. He had observed the smaller monkeys, and concluded that they seized and wrought indifferently with either hand, but the larger anthropoid apes he had not observed.

<center>*November* 15, 1875.</center>

<center>PROFESSOR BABINGTON, VICE-PRESIDENT, in the Chair.</center>

The following communications were made to the Society:

(1) *On the behaviour of Nucleus during Segmentation. By* F. M. BALFOUR.

The following observations were made upon the eggs of Scyllium and Pristiurus. At a late stage of the segmentation of these eggs most of the segments contain nuclei, but in some of them there is to be seen in the place of the nucleus a peculiar body. This has the shape of two cones with their bases in apposition. In each cone a series of striæ radiate from the apex to the base; and between the two is an irregular row of granules. From the apex of the cone there further diverge into the protoplasm of the cell a series of lines. The author regards these peculiar bodies as metamorphosed nuclei in the act of dividing. He points out that the simple division of the nucleus, as well as its complete disappearance, accompanied by the formation of two fresh nuclei, are well-authenticated modes of behaviour of the nucleus during cell-division. These two processes can only be connected on the supposition that in the second case the two fresh nuclei are formed from the matter of the old nucleus. The author considers that there exist in Selachians modes of behaviour of the nucleus intermediate

between the two extremes mentioned above, and points out
that in the peculiar striation of the body he described there are
indications of the streaming out of its matter into the sur-
rounding protoplasm; while on the other hand it never com-
pletely vanishes. It therefore affords an instance where part of
the matter of the nucleus divides and part streams out into the
protoplasm of the cell to be again collected to assist in the
formation of two fresh nuclei. The author further states that
he has found other bodies intermediate between the cone-like
bodies mentioned above and true nuclei; and regards these also
as nuclei in the act of division, where a still larger bulk of the
protoplasm of the nucleus becomes divided and a smaller part
rises with the surrounding protoplasm.

(2) *On the effects of Upas Antiar on the Heart.*
By M. FOSTER, M.A., F.R.S.

The author recording the movements both of the ventricle
and the auricles of a frog's heart (Rana temporaria), within
the body, by means of two delicate levers, observed in addition
to the well-known phenomena of antiar poisoning, a marked
slowing of the rythm in the later stages of the action of the
poison. The prolongation of each systole was also distinctly
marked, especially in the case of the auricles, which, much
distended in consequence of the partial occlusion of the con-
tracted ventricle, caused the lever resting on them to make an
enormous excursion at each systole. So long as any beat was
capable of being recorded by the lever resting on the ventricle,
the ventricular systole occurred in its proper sequence. Though
the whole rythm often became irregular, the phases of each
cardiac cycle remained regular.

Repeating the experiment of Schmiedeberg (*Beit. zu Anat.*

u. Phys.: also *Festgabe,* C. Ludwig *Gewidmet,* p. 222), the author found that when the ventricle had apparently ceased to beat, forcible distension of its cavity with a normal solution brought back a temporary series of pulsations ; but this restoration is possible only within a narrow range of time, and as Schmiedeberg himself seems to admit, cannot be regarded as shewing that the poison's chief action consists in preventing the normal muscular relaxation following upon each systole.

Repeating Neufeld's observation (*Stud. Phys. Inst., Breslau,* III. p. 97) the author found that strong solutions of potassium cyanide would sometimes restore the beat for a short time— but in this case also the phase at which this could be effected was very transient and very frequently failed, and inasmuch as such solutions are capable of stimulating muscular tissue directly, he was led to the conclusion that the restoration when obtained is not due to any relaxing action of the cyanide but to its chemical stimulation of the cardiac muscles.

When the vagus is stimulated in the earlier stages of antiar poisoning, inhibition is obtained as usual, but is followed by a somewhat lengthened period in which the beats are both more rapid and more forcible.

When the antiar has produced such an effect on the heart that its beats are exceedingly feeble and hardly capable of being recorded, this secondary action of vagus stimulation becomes exceedingly marked, the pulsations during its continuance being as forcible or even more forcible than normal, and at the same time rapid.

Lastly, a stage of poisoning may be witnessed when the ventricle is apparently at rest (i.e. not pulsating at all as far as the eye can judge, though of course in the contracted state so characteristic of antiar), where stimulation of the vagus produces no inhibition (for there is no beat to stop) but is followed by a lengthened series of often very vigorous and rapid pulsations. The author could not satisfy himself that *during* the

stimulation of the vagus any *relaxation* of the contracted ventricle took place, but on this point he is not sure.

These results at first sight seem identical with the phenomena observed by Schmiedeberg in nicotin poisoning (Ludwig's *Arbeiten,* 1870, p. 41), and explained by him as due to *accelerator* fibres in the vagus of the frog.

The author is unable to accept this explanation :

1. Because both inhibition and secondary action fail when atropin is given with the antiar (the antiar otherwise acting as usual).

2. Because a similar secondary action may be seen in all cases of inhibition not only of the frog's heart but also of the mammalian heart in which the accelerator fibres are supposed to run *not* in the vagus.

3. Because a similar secondary action may be seen in the snail's heart after inhibition by direct application of the interrupted current, and in certain conditions of the snail's heart may be witnessed when the inhibition cannot be detected.

The author regards the secondary action as being what for want of more precise knowledge he would call " a reaction " following the direct action of the vagus. And considering that antiar acts essentially, or at least primarily, on the muscular tissue of the heart, the peculiar prominence of this reaction in antiar poisoning may be taken as indicating on the one hand that the effects of antiar are especially favourable to this reaction, and on the other that the vagus nerve brings about inhibition by acting directly on the muscular tissue itself—a view which is supported by other facts.

November 29, 1875.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society:

On the temperatures observed in a deep boring at Speremberg near Berlin, as given in a report of a paper by Professor Mohr, of Bonn, in 'Nature' of October 21, 1875. *By* MR O. FISHER.

The greatest depth recorded is 3390 feet. The temperatures are given in Reaumur's scale. The author shewed that the equation

$$v = -\frac{251}{10^8} x^2 + 0.012982\, x + 7.1817,$$

in which v is the temperature, and x the depth, exactly represents the temperature curve. This curve would give a maximum temperature of

$$40°.7532\ \text{R., or}\ 123°.6947\ \text{Fah.,}$$

at a depth of 5171 feet. If there was no cause to disturb the temperature, it ought to conform to a straight line, given by the above equation altered by omitting the term in x^2. Consequently a cause was sought which would change such a straight line to the parabolic form. The first cause examined was a change in the conductivity of the strata depending on the depth, and it was found that a law, which would make the conductivity vary inversely as the distance of any point above the level of greatest temperature, would account for the observed facts. But it was argued that such a law was entirely improbable.

The next cause examined was the effect of the descent of water through the strata, and the author believes that this circumstance will account for the observed temperatures.

It was remarked that the results of this investigation make it appear, that the true law of underground temperature would be better obtained from borings of moderate than of very great depth, because the disturbance of the temperature curve from the rectilinear form is greater the further we descend.

April 19, 1875[1].

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.
On the Physiological Action of Jaborandi. By
Mr J. N. LANGLEY.

[*Abstract.*]

The preparations used are

(1) The alcoholic extract of the crushed Jaborandi leaves.

(2) The glycerine solution of this extract evaporated to dryness.

The results of experiments point to there being more than one active principle.

Injected subcutaneously there is one striking difference in the action of Jaborandi on the Frog and the Rat. In the former it causes convulsive movements with occasional tetanic spasms, in the latter it acts as a narcotic. Death in one case is probably more immediately caused by the stopping of the heart's beat; in the other by paralysis of the respiratory centre.

In the Frog convulsive movements are noticeable if any part of the spinal cord be left intact, but not otherwise.

Reflex action is greatly depressed.

Jaborandi has little or no effect on nerves, their endings in striated muscle, or on striated muscle itself.

If an arterial blood-pressure tracing be taken of a Mammal, and Jaborandi injected into a vein, it causes

(1) A fall in the blood-pressure.

(2) A slowing of the pulse.

(3) Generally a flattening of the respiratory curves.

[1] Omitted, by accident, from the last Number of the " Proceedings."

(1) The character of this depends very largely upon the method of injection, the rapidity of fall being directly as the rapidity of introduction into the blood.

The blood-pressure continues lower for two or three hours—the longest time during which any experiment was carried on. It may be lowered to a very considerable extent, less than one-half.

The fall is largely due to dilatation of the small blood-vessels, since

(a) After injection of Atropin the blood-pressure does not rise though the slowing of the heart is removed.

(b) After injection of Atropin the blood-pressure is still further reduced by a fresh injection of Jaborandi, though the heart-beat rate remains the same.

(c) When the Jaborandi is carefully and slowly injected, the tracing of the fall of blood-pressure is not recognisable from that obtained by stimulation of the central end of the Depressor.

The fact that after the blood-pressure has been very considerably lowered by Jaborandi, stimulation of the central end of the depressor gives a further lowering, points to its being due, not to a central paralysis of the vaso-motor centre, but to some local action : moreover Jaborandi causes dilatation in the blood-vessels of the Frog's web after section of the sciatic.

(2) The slowing is not due to a stimulation of the cardio-inhibitory centre, since it takes place in the Frog after complete destruction of the brain and spinal cord, and in the Frog after section of both vagi.

In the Frog there is an increased susceptibility to inhibition of the heart by vagus stimulation after giving Jaborandi.

With a moderate dose and after some time, stimulation of the sinus venosus still produces inhibition, though with a larger dose, and after some time, it no longer does.

Atropin removes the slowing rapidly and effectually, the

heart of a frog which has ceased beating may be caused to beat again by putting a few drops of dilute solution of Atropin on it.

As the heart regains its rapidity of beat, the inhibitory power of the pneumogastric is lost.

Since the slowing takes place after a large dose of Curari, it probably acts more peripherally than the endings of the vagus nerves.

Some curves obtained after a dose of Jaborandi very strongly suggest that they are due, not to the ordinary effects supposed to give the respiratory curves, but to a rhythmic increase in the force of the heart-beat.

Some tracings show a want of correspondence between the 'respiratory curves' of the blood-pressure and the respirations: it is hoped that further inquiry into this and some other deviations from the normal tracings may throw some further light on the causes of the rhythmic variations in blood-pressure.

(3) The flattening of the respiratory curve is probably very largely, if not entirely, a mechanical effect of the slowing of the heart-beat; a like flattening is obtained by a weak stimulation of the pneumogastric.

Whether Jaborandi, apart from the alcohol or glycerine in which it is dissolved, can produce a local stasis, is doubtful but not impossible.

Jaborandi causes in Mammals at first a quickening of the respiration, then a slowing, then a paralysis of the respiratory centre : in one rabbit (under chloral) in which respiration had been stopped by a slow injection of Jaborandi, it was restored after two or three minutes of artificial respiration.

With regard to the effect on the secretions, only a few experiments have been made more than mentioned in a Preliminary Notice last February. In Mammals the lachrymal and salivary secretion are always increased, but in no case has it been observed to cause sugar in the urine.

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society:

On the effect of the constant current on the Heart.
By Mr FOSTER *and* Mr DEW SMITH.

THE VICE-PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On Bow's method of drawing diagrams in graphical statics, with illustrations from Peaucellier's linkage. By J. CLERK MAXWELL, M.A., *Professor of Experimental Physics.*

The use of Diagrams is a particular instance of that method of symbols which is so powerful an aid in the advancement of science.

A diagram differs from a picture in this respect, that in a diagram no attempt is made to represent those features of the actual material system which are not the special objects of our study.

Thus when we are studying the internal equilibrium of a particular piece of a structure or a machine, we require to know its shape and dimensions, and the specification of these may often be made easier by means of a drawing of the piece.

But when we are studying the equilibrium of a framework composed of such pieces jointed together, in which each piece acts only by tension or by pressure between its extremities, it is not necessary to know whether a particular piece is straight or curved or what may be the form of its section. In order, therefore, to exhibit the structure of the frame in the most elementary manner we may draw it as a skeleton in which the different joints are connected by straight lines. The tension or pressure of each piece may be indicated on such a diagram by numbers attached to the line which represents that piece in the diagram. The stresses in the frame would thus be indicated in a way which is geometrical as regards the position and direction of the forces, but arithmetical as regards their magnitude.

But a purely geometrical representation of a force has been made use of from the earliest beginnings of mechanics as a science. The force is represented by a straight line drawn from the point of application of the force, in the direction of the force, and containing as many units of length as there are units of force in the force. The end of the line is marked by an arrow-head to show in which direction the force acts.

According to this method each force is drawn in its proper position in the diagram which represents the configuration of the system. Such a diagram might be useful as a record of the results of calculation of the magnitude of the forces, but it would be of no use in enabling us to test the correctness of the calculation. It would be of less use than the diagram in which the magnitudes of the forces were indicated by numbers.

But we have a geometrical method of testing the equilibrium of any set of forces acting at a point by drawing in series a set of lines parallel and proportional to these forces. If these lines form a closed polygon the forces are in equilibrium. We might thus form a set of polygons of forces, one for each joint of the frame. But in so doing we give up the principle of always

drawing the line representing a force from its point of appli-
cation, for all the sides of a polygon cannot pass through the
same point as the forces do.

We also represent every stress twice over, for it appears as a
side of both the polygons corresponding to the two joints be-
tween which it acts.

But if we can arrange the polygons in such a way that the
sides of any two polygons which represent the same force coin-
cide with each other, we may form a diagram in which every
stress is represented in direction and magnitude, though not in
position, by a single line, which is the common boundary of the
two polygons which represent the points of concourse of the
pieces of the frame.

Here we have a pure diagram of forces, in which no attempt
is made to represent the configuration of the material system,
and in which every force is not only represented in direction
and magnitude by a straight line, but the equilibrium of the
forces is manifest by inspection, for we have only to examine
whether each polygon is closed or not.

The relations between the diagram of the frame and the
diagram of stress are as follows:

To every piece in the frame corresponds a line in the
diagram of stress which represents in magnitude and direction
the stress acting on that piece.

To every joint of the frame corresponds a closed polygon in
the diagram, and the forces acting at that joint are represented
by the sides of the polygon taken in a certain cyclical order.
The cyclical order of the sides of two adjacent polygons is such
that their common side is traced in opposite directions in going
round the two polygons.

When to every point of concourse of the lines in the diagram
of stress corresponds a closed polygon in the skeleton of the
frame, the two diagrams are said to be reciprocal.

The first extensions of the method of diagrams of forces to

other cases than that of the funicular polygon were given by Rankine in his *Applied Mechanics* (1857).

The method was independently applied to a large number of cases by Mr W. P. Taylor, a practical draughtsman in the office of the well-known contractor Mr J. B. Cochrane. I pointed out the reciprocal properties of the diagram in 1864, and in 1870 showed the relations of this method to Airy's function of stress and other mathematical methods.

Prof. Fleeming Jenkin has given a number of applications of the method to practice, *Trans. R. S. E.*, Vol. XXV.

Cremona[1] has deduced the construction of the reciprocal figures from the theory of the two linear components of a wrench.

Culmann in his *Graphische Statik* makes great use of diagrams of forces, some of which, however, are not reciprocal.

M. Maurice Levy in his *Statique Graphique* (Paris, 1874) has treated the whole subject in an elementary and complete manner.

Mr R. H. Bow, C.E., F.R.S.E., in a recent work *On the Economics of Construction in relation to Framed Structures* (Spon, 1873), has materially simplified the process of drawing a diagram of stress reciprocal to a given frame acted on by any system of equilibrating external forces.

Instead of lettering the joints of the frame as is generally done, or the pieces of the frame as was my own custom, he places a letter in each of the polygonal areas enclosed by the pieces of the frame, and also in each of the divisions of the surrounding space as separated by the lines of action of the external forces.

When one piece of the frame crosses another, the point of intersection is treated as if it were a real joint, and the stresses of each of the intersecting are represented twice in the diagram of stress, as the opposite sides of the parallelogram which repre-

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[1] *Le figure reciproche nella statica grafica* (Milano, 1872).

sents the forces at the point of intersection. Thus the point V in figures 1 and 3, p. 412, is represented by the parallelogram $BCDE$ in figure 2, and the point A in figure 2 is represented by the parallelogram $PRQS$ in figures 1 and 3.

Peaucellier's linkage consists of the four equal pieces forming the jointed rhombus $PQRS$ together with two equal arms OS and OR.

When these arms are longer than the sides of the rhombus the linkage is said to be positive; when they are shorter the linkage is said to be negative.

When Peaucellier's linkage is employed as a machine it is acted on by three forces, applied respectively at the fulcrum O, and the two tracing poles Q and S.

These three forces, if in equilibrium, must meet in some point T. We may therefore suppose them to be stresses in three new pieces OT, QT, ST, which will complete the frame.

Let us suppose that both O and T are outside the rhombus, and that OS intersects PT in the point V, and let us apply Bow's method to construct the diagram of stress reciprocal to this frame.

If we letter the areas as follows, putting

A for the rhombus $PRQS$,

B for the triangle PSV,

C for the triangle OTV,
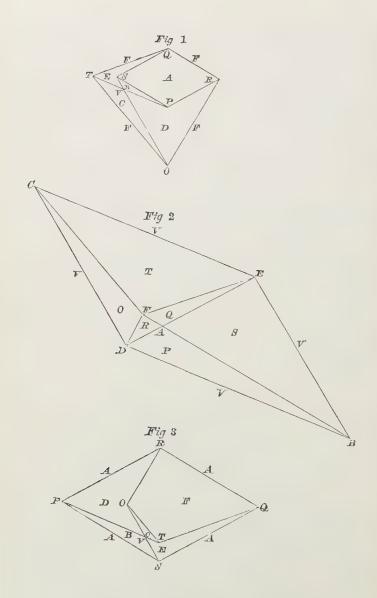
D for the quadrilateral $ORPV$,

E for the quadrilateral $QSVT$,

and F for the space outside the frame,

then, in the diagram of stress, the stresses of the four sides of the rhombus will meet in A, and since the opposite sides of the rhombus are parallel, the lines EA and AD will be in one straight line, and the lines BA and AF will also be in a straight line.

Also since in the frame the pieces OR and OS are equal, the angles ORP, PSV are equal, and the corresponding angles

Fig 1

Fig 2

Fig 3

FDA, ABE must be equal, and therefore the quadrilateral $BEFD$ can be inscribed in a circle, and therefore the angles FEA, DBA are equal, and the corresponding angles in the frame TQS, SPV are equal, and therefore PT is equal to QT.

If, therefore, O is in one diagonal of the rhombus, T must be in the other diagonal.

The diagram of stress is completed by drawing EC parallel to BD, and DC parallel to BE, and joining FC.

This diagram therefore consists of a parallelogram $BDCE$, a diagonal ED, a point F in the circle passing through FBD, and four lines drawn from F to the angles of the parallelogram.

If we now begin with the diagram of stress, and proceed to construct a frame reciprocal to it, the form of the frame will be different according to the cyclical direction in which the sides of the rhombus $PRQS$ are lettered. If in the one case we have the points O and T both outside the rhombus as in fig. 1, in the other O and T will both be within the rhombus as in fig. 3. The stresses in the corresponding pieces of fig. 1 and fig. 3 are all equal if they are equal in any pair of them.

If in the frames represented in fig. 1 and fig. 3, we consider that the pieces OS and TP cross one another at V without intersecting, we have six points O, P, Q, R, S, T joined by nine lines. Now in general if p points in a plane are joined by $2p - 3$ lines the figure is simply stiff, that is to say the form of the figure is determined by the lengths of the lines, and there are no necessary relations between the lengths of the lines.

But in Peaucellier's linkage the length of any line, as OT, is determined when those of the other eight are given. For if a is the length of a side of the rhombus, b the length of either arm OR or OS, c the length of either arm TP or TQ, then if $OT = d$,

$$d^2 = b^2 + c^2 - a^2.$$

Hence if any one of the nine pieces of the linkage be removed, the motion of the remaining eight will be the same as

before, and a given stress in any one of the nine will produce stresses in each of the other eight which are determinate in magnitude when the configuration of the linkage is given, though they alter during the motion of the linkage.

March 13, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society :

On a set of Lunar Distances. By Mr PEARSON.

In this paper I propose to discuss the anomalies exhibited by a set of Lunar distances which I took under rather peculiar circumstances last autumn. The sky was perfectly clear, with the exception of a light cloud touching the sun, at the commencement : and the horizon quite open. The position of observation was, by the ordnance survey, as near as possible Lat. 52°. 7′. 13″ N., Long. 56ˢ. (14′) E. The instrument with which they were taken is the same prism-circle, 6 inches in diameter, by Pistor and Martins, of Berlin, which was shewn on a previous occasion at a meeting of the Society, and with which I took the observations given pp. 351—354, and pp. 357—359 of this vol. of the *Proceedings*.

It will be well first to consider the condition of the instrument itself, as it may be suggested that the source of the anomalies is to be found here. Being graduated all round, the mean of the opposite readings has in all cases been taken. Between these there is, in the first case, a discrepancy between the opposite readings of two divisions of the vernier, or 40″ about ; for (2) we have a discrepancy of 35″ ; (3), (4), (5), and (7) give the same readings on both sides of the arc ; (6) gives a discrepancy of 20″. At 101° and at 102° there is no discrepancy between the opposite readings : so that it is clear that

the error in the first two cases is only casual: and also, what is of more importance, that the centering is very nearly accurate; and though it is just possible that the large error in the measured distance in the two first cases is due to bad graduation, it is clear that the fault extends to both sides of the instrument. The index-error (additive) at the zero-point having remained constant for some months at about 10″, this amount has been added in all cases. Having thus shewn that the anomalies can only be due to a fault in the graduation on the corresponding opposite sides of the instrument, such as can only be accurately estimated by a long series of observations and comparison with other instruments, I will proceed to give the elements of the observations themselves.

	G.M.T.	L.M.T.	⊙ app. alt.	⊙ true alt.	☽ app. alt.	☽ true alt.
	h. m. s.	h. m. s.	° ′ ″	° ′ ″	° ′ ″	° ′ ″
(1)	4.32.26	4.33.22	6.37.20	6.29.38	5.43.32	6.29.37
(2)	4.39. 4	4.40. 0	5.40.20	5.31.23	6.13.13	6.59.50
(3)	4.41.18	4.42.14	5.20.58	5.11.38	6.22.52	7. 9.40
(4)	4.44. 3	4.44.59	4.57.24	4.47.18	6.34.50	7.21.50
(5)	4.46.35	4.47.31	4.35.33	4.24.45	6.47.39	7.34.52
(6)	4.53.26	4.54.22	3.36.45	3.23.45	7.13.40	8. 1.12
(7)	4.55.22	4.56.18	3.20.13	3. 6.28	7.21.10	8. 8.50

	Obs. dist. of cents. of ⊙ & ☽.	Reduced dist. of do.	Error in time.	Error in arc.
	° ′ ″	° ′ ″	m. s.	′ ″
(1)	101.26.50	101.21.27	5.34	2.36
(2)	101.29. 8	101.24.36	5.31	2.34
(3)	101.30.40	101.26.26	3.35	1.38
(4)	101.32.20	101.28.30	1.59	0.56
(5)	101.32.10	101.28.42	4.46	2.13
(6)	101.35.20	101.32.58	2. 8	1. 0
(7)	101.36. 0	101.33.58	1.44	0.49

As the G. M. T. was obtained by comparing the watch with a carefully rated chronometer immediately after the observation, I am certain that it is not more than 5ˢ to 10ˢ, at the most, out.

We have here a set of 7 observations, the moon to the left of the meridian about 2 hours, the sun to the right about $4\frac{1}{2}$ to 5 hours.

The moon at first low. App. alt. 5°. 43`.

The sun at the same time higher. App. alt. 6°. 37`.

The moon at the last had risen 1°. 40`.  App. alt. 7°. 21`.

The sun   ,,   ,,   fallen 3°. 45`.    ,,    3°. 20`.

The fourth observation I suspect, but this I need not discuss.

Now, firstly, the mean of the first two gives the place of observation $5^m. 32\frac{1}{2}^s$ to the East of its true place, *that* of the last two diminishes this error to 55^s; but as, taking the mean of the first two, the 4th and 5th, and the 6th and 7th, the errors marked by them are in the proportion of $5\frac{1}{2}$, $3\frac{1}{2}$, $3\frac{1}{3}$, 2, nearly; a mean of them all cannot fairly be taken.

Secondly: all these observations give an earlier Greenwich, and therefore local time, than the true time; because, allowing that the distances tabulated in the *Nautical Almanac* are correct, they make the distances between the sun and moon less than they ought to be, as the moon was receding from the sun at the time.

Query. Can this be the error of the *instrument?*

It cannot be the *Index Error*, commonly so called, because I repeatedly examined this under the most favourable circumstances, by taking the distance between the two opposite (horizontal) edges of the sun.

It cannot be from simple *false centering*, because, at this point of the graduated arc, there is so small a discrepancy between the readings on the two sides, and that not continuous.

In order to discuss the matter properly, I will refer to some other examples.

First set of examples, which I have taken since.

The moon and Venus (Mar. 1, p.m.) both to *right* of meridian (moon's H.A. $2\frac{1}{2}$ hours, Venus's $4\frac{1}{2}$ about) give

Observed   dist. 31°. 39'. 35" $\left\{\begin{array}{l}\text{error in arc 1'. 40" (abt. 3}^{m}\text{. 8}^{s}\\ \text{time); too small, as in the case}\\ \text{of the sun, already given.}\end{array}\right.$
Computed dist. 31°. 41'. 15"

Also the sun, in the previous example; the planet *in this*, are to the right of the moon: but the moon, in this case, is at a much *greater* altitude.

But for three cases of *Pollux* and the *moon*, the moon in this case being to the *right* of the star, not to the left, as before, I obtained

	1.	2.	3.
Obs. dist.	32°. 38'. 15"	30°. 39'. 4"	30°. 33'. 46"
Comp. dist.	32°. 37'. 31½"	30°. 37'. 42"	30°. 32'. 59"
	43½"	1'. 22"	47"
	(abt. 1ᵐ. 20ˢ time.)	(abt. 2ᵐ. 20ˢ time.)	(abt. 1ᵐ. 20ˢ time.)

In all these cases the *observed distance is too great;* but the moon and star in the 1st case were on opposite sides of the meridian; in the latter two cases only, the H.A. of the moon was considerable. With these I couple a case of Mars and the moon (H.A. of Mars 1ʰ. 50ᵐ, of moon 3ʰ. 15ᵐ), both to right of meridian; because the error agrees with that in the case of Pollux in a similar position; the computed distance (but not given in the N. A.) at a certain time (Jan. 29, 1876) being 23°. 29'. 13", and the reduced measured distance 23°. 30'. 6", or nearly 1' too great.

In another example (Sept. 7, 1875, p.m.), with the moon and sun very low (less than 10°), and the former almost on the meridian, I had an error of 1'. 44" arc, or 3ᵐ. 41ˢ time. In this case again the observed distance was too small, and the sun, of course, to the *right* of the moon.

I may mention that Capt. Parry in fixing his Long. at Port Bowen 88°. 54'. 55" E.—73°. 13'. 39" N. Lat., made his Long. by 6 occultations of fixed stars to differ from the mean of all his observations by 4" only.   But by

Moon's transits (a large number) ...	2'. 42".
By eclipses of Jupiter's satellites ...	2'. 40".
By chronometers........................	20".
By lunar distances	26".

Again, he took 620 sets (310 E., 310 W.), and the extreme error is as much as 32'.   As 10 went to a set, it is possible that a part of the error arose from taking so many together.

Lastly, I will mention one very accurate observation of the moon and Jupiter, May 10, 1875 (dist. 81°. 30'. 30"), which gave the Long. of Emmanuel College 45^s E., or only 15^s too much. Here the observed distance is about 10", or one division of the vernier too large, but the error is too small to notice.

I may conclude by saying that in a few other cases of Lunars, when the moon has been nearly on the meridian, and at an altitude of at least 30°, I have not found any error except what might be properly ascribed to defects in my own power of observing, or a local error in the graduation of the circle; also that we may be sure that the large error in the first observations cannot well have been due to an error in the estimated amount of refraction, as the thermometer and barometer had been observed, Th. 48°, Bar. 29°. 9, about cancelling one another.   (See on this point *Ast. Soc. Month. Not.* Vol. XXIII. p. 58; Zach. *Monat. Corresp.* XXVII. p. 341; Shortrede's Log. Tables (ed. 1844), Introd. p. 12; Brinkley in *Memoirs of R. I. A.* XIII. p. 170.)   Nor has it escaped my notice that had the changes in the *reduced* distance (by Borda's formula, I believe, see *ante*, p. 358) agreed with those in the *observed* distance, the error which I have discussed would not nearly have amounted to so much.

March 27, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communications were made to the Society:

On the relation of the Spinal Cord to the Tail in Mammals. By Mr ANNINGSON.

After noticing the varying position of the spinal cord and its nerves at different ages in man, and quoting some anatomical works in reference to the length of the spinal cord and the position of its nerves in long- and short-tailed mammals respectively, the speaker proceeded to shew that some of the statements contained in the books were not quite in accord with the evidence of his own dissections. The facts which he wished to point out were (1) The constancy of a cauda equina and filum terminale in both long- and short-tailed mammals; (2) The superficial position attained by the filum terminale towards the end of the tail; (3) The general constancy in the absolute number of sacro-caudal nerves irrespective of the total number of sacro-caudal vertebræ; (4) The direct relation between the number of sacro-caudal spinal nerves and the number of ossified neural arches. He concluded by pointing out the relation the above facts might bear to the development of the tail in the individual and in the mammalian series.

On Vital Force. By Mr H. F. BAXTER.

After a few preliminary observations the author proposes the following question, What is the nature of Vital Force? By vital force he means the *force* that is manifested during the growth, development, or evolution of organised bodies, both plants and animals. Can we associate this *force* with any other

known *form* of force ? Do the actions, such as *secretion, nutri-
tion* and *absorption*, which take place in an organised body,
differ from those which occur in the laboratory of the chemist ?

Reference is then made to the well-known conjecture and
experiment of Wollaston which occurred so far back as 1806,
and originated at the time Davy made his celebrated discovery
of the decomposition of chemical compounds by means of the
voltaic battery. In Wollaston's experiment, a weak solution of
common salt was used and the soda made to transude through
a membrane, the metals employed being zinc and silver, form-
ing an elementary circle. "The efficacy of powers," says
Wollaston, "so feeble as are here called into action, tends to
confirm the conjecture that similar agents may be instrumental
in effecting the various animal secretions which have not yet
been otherwise explained." Wollaston's conjecture was so far
true, but it is necessary to be acquainted with Faraday's views
in regard to the *origin* of the power in the voltaic circle ; the
current not being the *cause* of the power, but a manifestation
of the chemical action that is taking place. Allusion is then
made to Becquerel's experiments to show that metals are not
necessary to obtain current force; ordinary chemical actions,
such as the combination of an acid with an alkali, will pro-
duce it.

The author then refers to the results of his own experiments
with regard to *secretion*, first in the *stomach* and *intestines ;*
secondly with the *biliary secretion ;* thirdly with *urinary secre-
tion*, and fourthly with *mammary secretion*. In all these cases
the electrode in contact with the *venous blood* flowing from the
part was *positive* to the electrode in contact with the *secreted*
product. To account for these results according to Becquerel's
experiments we should be obliged to assume that the venous
blood was acid to the secreted product, and not only that, but that
just after the separation had taken place a *combination* occurred
between the secreted product and the blood. As this reasoning

organic force be considered as a polar force also, as the essential conditions for current force exist in both cases.

Reference is next made to the results obtained during *lacteal absorption,* and those during *nutrition* in the muscular and nervous tissues, and to his experiments upon the roots and leaves in plants. In all these cases, although the results are not so decisive as in the case of secretion in animals, they nevertheless indicate that similar results may be obtained. The author then sums up with the following remarks. Let me now state the general conclusions that the results of all these experiments lead me to infer. The animal body is not a voltaic battery, nor an electro-magnetic machine, nor a steam-engine. I have been *comparing* the actions which take place in the voltaic circle and those which occur in the organised body, and find they are identical in all their essential conditions; that the existence of current force which is manifested in the one case exists also in the other; that the *direction* of the current depending upon the direction in which the *anion* and *cation* move in the circle is the same in both instances. If the manifestation of current-force be evidence of *polar* actions in one case so must it be in the other; if chemical force be polar so must organic force be polar. I am now talking of organic force, the title of my paper is vital force. Vital force and organic force to my mind are identical terms; vital force may perhaps be more comprehensive as embracing the action of the nervous system. We can have life without nerves, as in plants. But what do I mean by the term life? By life I mean *the series of changes which take place in organic matter resulting in the development, or evolution, of an individual organism.* By an individual organism I mean a *monad,* a *man,* an *annual plant.*

The President (Professor Clerk Maxwell) in the Chair.

The following communications were made to the Society :

(1) *On the Friction attributed to the Ether.* *By* Mr W. M. Hicks.

In the Proc. Roy. Soc. Vols. xiv. xv. xxi. appear descriptions of some experiments of Stewart and Tait on the heating of a disk by rapid rotation in vacuo. The experiments were made with thin disks of aluminium and ebonite, the apparatus with a thermo-pile being placed inside a receiver and the receiver exhausted. On rotating the disk for 30″ or 40″ the pile showed a heating effect.

For aluminium disks of $\frac{1}{9}$ in thickness the effect on the pile corresponded to a rise of temperature of about ·85° F. and of $\frac{1}{40}$ in thickness to a rise of 1·7° F.

They showed that the heating effect is not due to either

1. Rotation under the Earth's magnetic force,
2. Nor conduction of heat from the bearings,
3. Nor to radiation or convection from the wheel-work,
4. Nor to vibrations of the disk.

In a second series of experiments, they found that the heating effect was due to two causes,

1. A residual gas effect,
2. An unknown effect.

This unknown effect they set down as due to etherial friction, but they arrive at this conclusion by a process of exhaustion, and hence they cannot be sure of having exhausted all other possible causes. And one cause, at least possible a priori, they do seem to have passed over.

The unknown effect to be explained has the following properties:

1. It is a surface-effect more deeply seated than the gas-effect.

2. It varies in the same proportion along the radius as the gas-effect (and is therefore probably proportional to the distance from the centre).

3. The quantity of heat produced in disks of different thicknesses appears the same, as the radiation was found to be proportional inversely to the thickness.

4. The effect is produced without perceptible diminution when the disk is covered with a chamois leather blind.

5. It is independent of the residual gas.

6. It is different for different disks.

Now the disk when rapidly rotating expands slightly, and consequently becomes cooled below the surrounding space. Hence during the time of rotation an equilibrium of temperature takes place and it becomes heated up towards its former temperature, when the rotation is stopped the disk shrinks to its former size and gives out the heat it had taken in whilst it was rotating. Now let us see how this explanation would satisfy the required conditions.

1. It is clearly a surface-effect when the rotation has not been continued too long, for the heat enters from outside and gets conducted inwards, and therefore it will be hotter outside than inside. It will be also more deeply seated than the gas-effect, for it is heated by radiation which affects a perceptible depth, while the gas-effect takes place on the surface only.

2. It can be shown that the displacement at any point of the disk is, when small, very nearly proportional to the distance from the centre, or $\xi = \mu r$. The strains are therefore $\dfrac{d\xi}{dr} = \mu$ and $\dfrac{\xi}{r} = \mu$ constant along and perpendicular to the radius. The

work done per unit of mass is therefore everywhere the same, and the temperature ought therefore to be uniformly raised all over the surface. This would imply that the gas-effect was uniform all over the disk, which is not probable. The explanation therefore does not seem to agree with experiment here, but unfortunately the experiment on which the law is based is rather unsatisfactory. In the first place, the temperatures were compared at two points only and distant from one another only $\frac{1}{5}$ radius; and secondly, the amount itself to be measured was very small.

3. The quantity of heat taken in would probably be the same for different thicknesses of the same kind of disk, for 40″ would not be long enough for even the thinnest disk to rise exactly to the surrounding temperature.

4. This would certainly be the case, though scarcely probable on the supposition of etherial friction.

5. It is clearly independent of the residual gas.

6. And it is evidently different for different disks. In fact, for india rubber it ought to give a cold effect.

If we consider what the etherial friction would be, it seems more probably due to a shearing force, separating the ether in the body from the ether in space than a true friction in the ordinary sense. If this were so, it would probably be to a great degree independent of the material of the disk ; but still it is clear that different materials would be differently affected, though the effect might not depend on the polish of the surface.

The question could at once be settled by the following experiments.

1. There ought to be at first, when the disk is in motion, a temporary cooling effect. (The heating effect was observed only when the disks were at rest.)

2. The work done at any point is proportional to the square of the angular velocity, while for friction it would

clearly be proportional to the angular velocity directly. Unfortunately the experiments were always made with the same angular velocity.

From Thermodynamic considerations we can show that the rise of temperature due to compression is

$$\Delta t = \left(\frac{c}{c'} - 1\right) \frac{\epsilon}{\mu} - \frac{1}{Jc'} \frac{dE}{dm},$$

where $c \cdot c'$ are specific heats at constant pressure and volume respectively, ϵ is the compression, μ the coefficient of expansion for heat, and $\frac{dE}{dm}$ the external work done per unit of mass.

It can be shown that the displacement at any point is given by

$$\xi = \frac{4\omega^2 a^3 m}{15\lambda} r = L \frac{m}{\lambda} \cdot r, \text{ say,}$$

where
$\lambda = $ coefficient of resistance to form,

$a = $ radius of disk,

$m = $ mass of unit of volume.

Work done at any point per unit mass

$$= \frac{\delta E}{dm} = \lambda \left(\overline{\left|\frac{d\xi}{dr}\right|}^2 + \overline{\left|\frac{\xi}{r}\right|}^2\right) \frac{\delta v}{\delta m} = \frac{2m}{\lambda} L^2.$$

The compression $\quad \epsilon = \frac{d\xi}{dr} + \frac{\xi}{r} = \frac{2m}{\lambda} \cdot L,$

whence $\qquad \Delta t = \left\{\left(\frac{c}{c'} - 1\right) \frac{2L}{\mu} - \frac{2L^2}{Jc'}\right\} \frac{m}{\lambda}.$

In the experiments

$$\omega = 2500 \text{ rev. in } 30'' = \frac{500\pi}{3},$$

$$a = 7\cdot5 \text{ in.} = \cdot1905 \text{ metres nearly,}$$

$$J = 424 \text{ gramme-metres.}$$

Substituting these values we find

$$\Delta t = \left\{ \left(\frac{c}{c'} - 1 \right) \frac{1010.7}{\mu} - \frac{1204.6}{c'} \right\} \frac{m}{\lambda}.$$

The units being gramme, metre, second, centigrade.

The experimental value of $\frac{c}{c'}$ for aluminium has not been found, but Prof. Maxwell has pointed out to me that Edlund has determined it for some other metals. So, though we are unable to calculate the amount of the heating for an aluminium disk, we may get an idea of its magnitude by taking some other metal. If we take silver we shall have

$$\frac{c}{c'} = 1.0203,$$
$$c = .057,$$
$$\mu = .000057,$$
$$\lambda = 8481 \times 10^9 \text{ grammes per square metre,}$$
$$m = 10.4 \times \text{mass of cubic metre of water,}$$
$$= 10.4 \times 10^6 \text{ grammes.}$$

Substituting these we find for a silver disk under the conditions of the experiment,

Fall of temperature on expanding $= .4°$ C.

As the conductivity of silver is very high, the heat absorbed during rotation would be rapidly conducted inwards, and therefore after 40″ the disk will almost have risen to the surrounding temperature, and consequently on stopping the disk we should get the whole effect of .4° C. showing itself. If we take into consideration the effect of the conduction at the end of 40″, the surface would be .04° C. below the surrounding space, and therefore on stopping the disk the temperature observed ought to be .36° C.

The order of magnitude of the effect is thus the same as

that of the experiments, and the explanation proposed seems sufficient to account for all the results.

If the heating is due to friction, the amount was shown to be about ·0006 lbs. per square feet, and that this would produce an alteration in the length of the day of not less than ·006″ in a century.

(2) On the Equilibrium of Heterogeneous Substances. By Prof. Clerk Maxwell.

The thermodynamical problem of the equilibrium of heterogeneous substances was first attacked by Kirchhoff in 1855, who studied the properties of mixtures of sulphuric acid with water, and the density of the vapour in equilibrium with the mixture. His method has recently been adopted by C. Neumann in his *Vorlesungen über die mechanische Theorie der Wärme* (Leipzig, 1875). Neither of these writers, however, make use of two of the most valuable concepts in Thermodynamics, namely, the intrinsic energy and the eutropy of the substance.

It is probably for this reason that their methods do not readily give an explanation of those states of equilibrium which are stable in themselves, but which the contact of certain substances may render unstable.

I therefore wish to point out to the Society the methods adopted by Professor J. Willard Gibbs of Yale College, published in the *Transactions of the Academy of Sciences of Connecticut*, which seem to me to throw a new light on Thermodynamics.

He considers the intrinsic energy (ϵ) of a homogeneous mass consisting of n kinds of component matter to be a function of $n+z$ variables, namely, the volume of the mass v, its eutropy η, and the n masses, $m_1, m_2 \ldots m_n$, of its component substances.

Each of these variables represents a physical quantity, the

value of which, for a material system, is the sum of its values
for the parts of the system.

By differentiating the energy with respect to each of these
variables (considered as independent), we obtain a set of $n + z$
differential coefficients which represent the intensity of various
properties of the substance. Thus,

$\dfrac{d\epsilon}{dv} = - p$, where p is the pressure of the substance ;

$\dfrac{d\epsilon}{d\eta} = \theta$, where θ is the temperature on the thermodynamic
scale ;

$\dfrac{d\epsilon}{dm_1} = \mu_1$, where μ_1 is the potential of the component (m_1) with
respect to the compound mass.

Each of the component substances has therefore a potential
with respect to the whole mass.

The idea of the potential of a substance is, I believe, due to
Prof. Gibbs. His definition is as follows :—

If to any homogeneous mass we suppose an infinitesimal
quantity of any substance to be added, the mass remaining
homogeneous, and its eutropy and volume remaining unchanged,
the increase of the energy of the mass, divided by the mass of
the substance added, is the *potential* of that substance in the
mass considered.

The condition of the stable equilibrium of the mass is ex-
pressed by Prof. Gibbs in either of the two following ways :

I. *For the equilibrium of any isolated system it is necessary
and sufficient that in all possible variations of the state of the
system which do not alter its energy, the variation of its eutropy
shall either vanish or be negative.*

II. *For the equilibrium of any isolated system it is necessary
and sufficient that in all possible variations of the state of the
system which do not alter its eutropy, the variation of the energy
shall either vanish or be positive.*

The variations here spoken of must not involve the transportation of any matter through any finite distance.

It follows from this that the quantities θ, p, $\mu_1...\mu_n$ must have the same values in all parts of the mass. For if not, heat will flow from places of higher to places of lower temperature, the mass as a whole will move from places of higher to places of lower pressure, and each of the several component substances will pass from places where its potential is higher to places where it is lower, if it can do so continuously.

Hence Prof. Gibbs shows that if Θ, P, $M_1...M_n$ are the values of θ, p, $\mu_1...\mu_n$ for a given phase of the compound, and if the quantity

$$K = \epsilon - \Theta\eta + Pv - M_1 m_1 - \&c. - M_n m_n,$$

is zero for the given fluid, and is positive for every other phase of the same components, the condition of the given fluid will be stable.

If this condition holds for all variations of the variables the fluid will be absolutely stable, but if it holds only for *small* variations but not for certain finite variations, then the fluid will be stable when not in contact with matter in any of those phases for which K is positive, but if matter in any one of these phases is in contact with it, its equilibrium will be destroyed, and a portion will pass into the phase of the substance with which it is in contact.

Thus in Professor F. Guthrie's experiments, a solution of chloride of calcium of 37 per cent. was cooled to a temperature somewhat below $-37°$ C. without solidification.

In this state, however, the contact of three different solids determines three different kinds of solidification. A piece of ice causes ice to separate from the fluid. A piece of the cryohydrate of chloride of calcium determines the formation of cryohydrate from the fluid, and the anhydrous salt causes a precipitation of anhydrous salt.

The phase of the fluid is such that K is positive for all phases differing slightly from its own phase, and its equilibrium is therefore stable, but for certain widely different phases, namely, ice, cryohydrate and anhydrous salt, K is negative.

If none of these substances are in contact with the fluid, the fluid cannot alter in phase without a transport of matter through a finite distance, and is therefore stable; but if any one of them is in contact with the fluid, part of the fluid is enabled to pass into a phase in which K is negative. The conditions of consistent phases are that the values of θ, p, $\mu_1 \ldots \mu_n$, and K are equal for all phases which can coexist in equilibrium, the surface of contact being plane.

This was illustrated by Mr Main's experiments on co-existent phases of mixtures of chloroform, alcohol and water.

MONDAY, *May* 22, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society :

On Curvilinear and Normal Co-ordinates. By the Rev. J. W. WARREN, M.A. (Communicated by Prof. CAYLEY.)

THE Memoir refers partly to the general theory of curvilinear co-ordinates, partly to the special case of normal co-ordinates.

Taking (u, v, w) each of them a given function of the rect-angular co-ordinates (x, y, z), so that a point is determined either by its rectangular co-ordinates (x, y, z) or by its curvi-linear co-ordinates (u, v, w), and writing

$$dx^2 + dy^2 + dz^2 = (\text{a, b, c, f, g, h})\,(du, dv, dw)^2,$$

and then, Ω being an arbitrary function of (x, y, z), or of (u, v, w),

$$\left(\frac{d\Omega}{dx}\right)^2 + \left(\frac{d\Omega}{dy}\right)^2 + \left(\frac{d\Omega}{dz}\right)^2 = (A, B, C, F, G, H)\left(\frac{d\Omega}{du}, \frac{d\Omega}{dv}, \frac{d\Omega}{dw}\right)^2,$$

then $(a, \dots)$, $(A, \dots)$ are given functions of the differential coefficients $\frac{dx}{du}$ &c., ... or $\frac{du}{dx}$ &c., that is of (x, y, z), or, what is the same thing, of (u, v, w), such that

$$A \quad : \quad B \quad : \quad C \quad : \quad F \quad : \quad G : H$$
$$= bc - f^2 : ca - g^2 : ab - h^2 : gh - af : hf - bg : fg - ch,$$

and

$$a \quad : \quad b \quad : \quad c \quad : \quad f \quad : \quad g \quad : \quad h$$
$$= BC - F^2 : CA - G^2 : AB - H^2 : GH - AF : HF - BG : FG - CH,$$

and the theory of curvilinear co-ordinates is in fact a theory of the mutual relations of these coefficients $(a, \dots)$ and $(A, \dots)$.

In Lamé's system of curvilinear co-ordinates where the surfaces $u = 0$, $v = 0$, $w = 0$ are orthotomic, $f = g = h = 0$, and therefore also $F = G = H = 0$: and the remaining coefficients correspond to Lamé's h, h_1, h_2, H, H_1, H_2; viz. we have

$$H = \frac{1}{h} = \frac{1}{\sqrt{A}} = \sqrt{a}, \quad H_1 = \frac{1}{h_1} = \frac{1}{\sqrt{B}} = \sqrt{b}, \quad H_2 = \frac{1}{h_2} = \frac{1}{\sqrt{C}} = \sqrt{c},$$

and Lamé gives six differential equations of the second order satisfied by h, h_1, h_2, or H, H_1, H_2, considered as functions of the variables which correspond to (u, v, w).

In the author's system of normal co-ordinates, u, v, w denote the normal distances of the point (x, y, z) from three given surfaces $u = 0$, $v = 0$, $w = 0$ respectively: and the coefficients are then such that $A = B = C = 1$. He obtains on this assump-

tion six differential equations of the second order satisfied by a, b, c, f, g, h considered as functions of (u, v, w); viz. the forms are

$$\frac{d^2b}{dw^2} + \frac{d^2c}{dv^2} - 2\frac{d^2f}{dv\,dw} = \text{given function of first derived functions,}$$

$$\vdots$$

$$\frac{d^2g}{du\,dv} + \frac{d^2h}{du\,dw} - \frac{d^2a}{dv\,dw} - \frac{d^2f}{du^2} = \text{given function of do.}$$

$$\vdots$$

and as a consequence of these he obtains a seventh differential equation of the second order, symmetrical as regards the coefficients (a, b, c), (f, g, h), and the variables (u, v, w); which seven equations are the chief analytical results arrived at in the memoir. The memoir contains various developments in relation to the curvature of the surfaces, &c.

HONORARY MEMBERS ELECTED.

Feb. 28, 1876.   Prof. Luigi Cremona, Rome.

March 13.   Joseph Prestwich, F.R.S., Prof. of Geology at Oxford.

NEW FELLOWS.

Feb. 14, 1876.   Edward Tanner, M.A., Christ's College.

  J. N. Langley, B.A., St John's College.

March 13.   W. M. Hicks, B.A., St John's College.

  G. T. Bettany, B.A., Caius College.

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